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THE BEHAVIORALIST MEETS THE MARKET:  
MEASURING SOCIAL PREFERENCES AND  
REPUTATION EFFECTS IN ACTUAL TRANSACTIONS

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The Behavioralist Meets the Market: Measuring Social Preferences and Reputation Effects in Actual Transactions

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**ABSTRACT**

The role of the market in mitigating and mediating various forms of behavior is perhaps the central issue facing behavioral economics today. This study designs a field experiment that is explicitly linked to a controlled laboratory experiment to examine whether, and to what extent, social preferences influence outcomes in actual market transactions. While agents drawn from a well-functioning marketplace behave in accord with social preference models in tightly controlled laboratory experiments, when observed in their naturally occurring settings their behavior approaches what is predicted by self-interest theory. In the limit, much of the observed behavior in the marketplace that is consistent with social preferences is due to reputational concerns: suppliers who expect to have future interactions with buyers provide higher product quality only when the buyer can verify quality via a third-party certifier. The data also speak to theories of how reputation effects enhance market performance. In particular, reputation and the monitoring of quality are found to be complements, and findings suggest that the private market can solve the lemons problem through third party verification.

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## I. Introduction

More than two decades ago, Stigler (1981) wrote that when “self-interest and ethical values with wide verbal allegiance are in conflict, much of the time, most of the time in fact, self-interest theory....will win.” While this is the conventional wisdom among economists, an influential collection of laboratory experiments on “gift exchange” has called into question the validity of Stigler’s position (see, e.g., Camerer and Weigelt, 1988; Fehr et al., 1993; Berg et al., 1995). This literature is complemented by an entire body of theoretical research exploring the economic consequences of “social preferences,” wherein agents have preferences that are measured over their own and others’ material payoffs (for models of reciprocity see Rabin, 1993, Dufwenberg and Kirchsteiger, 1999, Falk and Fischbacher, 1999, and Charness and Rabin, 2002; for models of inequity aversion see Fehr and Schmidt, 1999, and Bolton and Ockenfels, 2000; on altruism see Andreoni and Miller, 2002).<sup>1</sup> A second generation of experimental studies has subsequently emerged examining the nature of social preferences and underscoring the robustness of the gift exchange results (e.g., Charness, 1996; Fehr et al., 1997; Fehr and Falk, 1999; Charness and Rabin, 2002; Gächter and Falk, 2002; Hannan et al., 2004; Brown et al., 2004; Fehr and List, 2004).<sup>2</sup>

The gift exchange results, which are consistent with the notion that people behave in a reciprocal manner even when the behavior is costly and yields neither present nor future material rewards, have attracted much attention, as many have argued that they are relevant beyond the context inherent in the laboratory. For example, some view the experimental results as providing key support for the labor market predictions in Akerlof (1982) and Akerlof and Yellen, (1988; 1990), whereby higher than market-clearing wages and involuntary unemployment are potential

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<sup>1</sup> In this study, I explore social preferences under this broad definition and am not interested in pinpointing whether the behavior consistent with social preferences is due to altruism, reciprocity, fairness, inequality-aversion, or another motive. Yet within the gift exchange literature, reciprocity motives have been highlighted; thus, I will continue this spirit in the discussion below. For a parsing of trust and reciprocity in a laboratory experiment, see Cox (2004).

<sup>2</sup> Fehr and Gächter (2000) provide an overview. The interested reader should also see the related literature on “lemons” markets (e.g., Miller and Plott, 1985 and Holt and Sherman, 1990).

outcomes of fairness considerations in the workplace.<sup>3</sup> Indeed, Fehr et al. (1993, p. 437) note that their results “provide...experimental support for the fair wage-effort theory of involuntary unemployment.” Of course, social preferences might be important in many other strategic situations as well (for overviews see, e.g., Camerer, 2002, and Sobel, 2002), and therefore such results have broad implications for economists and non-economists alike. Despite these advances and the topic’s importance, it is fair to say that little is known about whether, and to what extent, social preferences influence economic outcomes in naturally occurring markets.<sup>4</sup>

The major goals of this study are to explore the nature of such preferences among real market players in both the laboratory and in naturally occurring environments. In doing so, the study provides a framework with which to disentangle social preferences and reputation effects. Measuring and disentangling social preferences and reputation effects is important in both a positive and normative sense, as optimal contracting and proposed government intervention in principal-agent settings, appropriate design of collective choice mechanisms, and theory-testing all depend critically on proper measurement of these effects. Equally important, the experimental design permits an examination of whether individual behavior in laboratory experiments provides a reliable indicator of behavior in the field—an issue fundamental to experimental economics.

To complete these tasks, I use several distinct experimental treatments to create a bridge between the laboratory and the field. A major attraction of this approach is that if behavioral differences are observed, I can pinpoint the important factors driving the disparities. I begin with a gift-exchange laboratory treatment (which is in effect a sequential prisoner’s dilemma game) that

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<sup>3</sup> This conjecture is typically termed the “fair wage-effort” hypothesis. Alternatively, note that the “efficiency wage theory” surmises that wages above market-clearing levels occur because these wage profiles induce workers to be motivated in an effort to avoid being fired, which economizes on firm-level monitoring (see, e.g., Katz, 1986).

<sup>4</sup> There is some survey evidence reported from interviews with managers that social preference considerations are important in the workplace (Blinder and Choi, 1990; Bewley, 1995). Furthermore, in a novel paper exploring the role of fairness in the marketplace, Kahneman et al. (1986) report results from telephone surveys of residents of two Canadian metropolitan areas (Toronto and Vancouver). They use a “dual entitlement” theory to explain their data: previous transactions establish a reference level of consumer and producer surplus, and fairness considerations arise from outcomes relative to these “entitlements.”

closely follows the received literature (e.g., Fehr et al., 1993). Rather than using student subjects, however, I make use of subjects drawn from a well-functioning marketplace—the sportscard market. In this setting, I place the experimental participants in their typical roles: consumers are placed in the role of buyers and dealers are placed in the role of sellers. This treatment is potentially important in that a fundamental feature of markets is that they sort agents into roles, whereas laboratory experiments randomly assign positions to agents. Experimental results are nevertheless consistent with the literature that uses students randomly allocated to roles: gift exchange is observed, and such behavior has an important influence on economic outcomes. This finding provides a validity check of the extant laboratory results on gift exchange, as it suggests that the major results can be replicated with real economic players from a much different population.

I proceed to explore several further treatments in the laboratory by varying the “context” in the experimental instructions—previous experimental studies typically use “context-free” instructions, such as neutral wording and avoiding words that might provide familiar contextual cues. Of course, this traditional approach potentially attenuates important elements of the exchange process, and therefore may suppress important psychological effects. A final laboratory treatment in this spirit moves a step toward the naturally occurring marketplace by creating an experimental lab market where buyers and sellers play a sequential prisoner’s dilemma game by exchanging cash for goods of uncertain quality in face-to-face transactions. If one ignores the artificiality invoked by the laboratory experimental setting, this particular treatment provides an environment that mirrors the actual decision-making process in the marketplace from which these subjects are drawn. As a whole, these design changes yield some behavioral differences, but gift exchange in these settings remains alive and well, both statistically and economically.

When moving from the lab to the field an important consideration is to remain parallel to the important lab features while ensuring the transaction is a natural one in the field. The field

experimental treatments mirror the laboratory gift exchange treatments and resemble many types of good or service markets: after receiving a price offer, sellers determine the good's quality, which cannot be perfectly measured by buyers. In the first field treatment, subjects approach dealers (who are unaware that they are taking part in an experiment) and offer either \$20 or \$65 for a sportscard of certain quality. Since quality is difficult to detect in this market for untrained consumers, and the approached dealers have a sufficient stock of cards on hand to provide the requested quality levels, if social preferences play a role in this case the card's grade and the price offer should be positively correlated. Once the buying agents had purchased each of the cards from the dealers, I had every card professionally graded. I do find a positive correlation between the prices and grades received, but only among dealers who are "locals"; among dealers who are likely to have little future interaction with the buying agents ("non-locals"), no such relationship emerges.

This result is interesting, but the data do not allow an unequivocal insight into the underlying mechanism at work. For instance, such data patterns might be due to several factors, including two competing alternatives critical to the issue at hand: selection effects—local dealers have social preferences and non-local dealers do not—or reputation effects—local dealers are concerned with their reputations whereas non-local dealers are not. A final set of three treatments in the marketplace provide insights into what is driving these behavioral differences by examining outcomes in an identical experiment for collector tickets and ticket stubs. Tickets and ticket stubs provide a unique test because *no* third-party verification service existed to grade tickets until June 2003, though the major grading company announced in April 2003 that they would soon begin grading tickets. By comparing temporal outcomes, I am permitted a unique opportunity to examine

not only the nature of market exchanges with and without third-party enforcement, but I am also able to explore the role of social preferences in such settings.<sup>5</sup>

In stark contrast to the results obtained from the sportscard data, the empirical results in the time period when no grading service was available and the public was unaware that a service was imminent (pre-April 2003), provides little evidence consistent with social preferences: ticket quality is not correlated with price for either dealer type, and local and non-local dealers provide similar quality levels. One could reason that dealers had little idea how to grade tickets since they had never been professionally graded (though many dealers made quality claims), and therefore the inability for this treatment to reject the homogeneity null is consistent with informational problems.

This potential drawback is rectified in an experimental treatment conducted after the announcement of grading (April 2003), but before the grading company released their grading criteria (June 2003). Purchasing identical tickets and using analogous protocol, I find that during this time period quality and price are correlated for tickets sold by local dealers but no correlation is present in ticket sales among non-local dealers. Completing the experimental design is an identical treatment conducted after grading services commenced (post-June 2003). Insights gained from this treatment are quite similar to those obtained from the treatment conducted between April and June: gift exchange is evident among local dealers but not among non-locals. This result stands to reason because PSA's ticket grading criteria is similar to its scheme for grading sportscards.

In summary, several insights follow. First, even though the data collected from one-shot laboratory experiments suggest that social preferences are quite important among this lot of agents, parallel treatments in the field suggest that such effects have minimal influence in naturally

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<sup>5</sup> Brown et al. (2004, p. 7) summarize the attractiveness of such treatments in motivating their laboratory experiments by noting "The ideal data set for studying the effects of the absence of third party enforceability on market interactions...is based on a truly exogenous ceteris paribus variation in the degree of third party enforceability.....The problem is, however, that it seems almost impossible to find or generate field data that approximates this ideal data set." This is exactly what these three treatments offer, and to my best knowledge such exogeneity has not heretofore been achieved in the literature.

occurring transactions. In this sense, dealer behavior in the marketplace approaches what is predicted by self-interest theory. From a methodological viewpoint, it is important to note that several changes to the laboratory environment had little influence on behavior, whereas moving from the lab to the field had striking effects. Second, empirical results provide insights into how reputation effects and professional certification influence market performance (see, e.g., Akerlof, 1970; Klein and Leffler, 1981). For example, I find that i) reputation effects enhance the quality of goods, and ii) reputation and the monitoring of quality are *complements*. In this spirit, the data suggest that the private market can solve the lemons problem through third party verification.

The remainder of this study is organized as follows. Section II describes the experimental design and summarizes the institutional details of the market. Section III provides a summary of the empirical findings, highlighting differences in results across the various treatments and describing the effects of reputation and social preferences on market outcomes across both local and non-local dealers. Section IV concludes with a discussion of the empirical results more generally.

## **II. Experimental Design and Hypotheses**

The experimental investigation begins with an examination of behavior in standard laboratory gift exchange games. Treatment Lab-R (R denotes laboratory replication—see Table 1 for a summary of the experimental design) makes use of the typical gift exchange experimental design.<sup>6</sup> One session was run in this treatment. In this session, each participant's experience typically followed four steps: (1) consideration of the invitation to participate in an experiment, (2) learning the experimental rules, (3) actual participation, and (4) conclusion of the experiment and exit interview. In Step 1, the monitor approached dealers on the floor of a sportscard show and inquired about their interest in participating in an economics experiment that would take about an

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<sup>6</sup> Appendix A contains a copy of the experimental instructions, which are closely related to Fehr et al. (1993; 1997) and Gächter and Falk (2002).



hour. If the dealer agreed, the monitor summarized the meeting time and place. A similar approach was used to recruit consumers (non-dealers).

Subjects met in a large room adjacent to the sportscard show floor: dealers entered on one side of the room and non-dealers on the other side, and a divider was in place to ensure that identities were not revealed. The session consisted of five periods, with five dealers acting as sellers and five non-dealers acting as buyers. Each participant received a copy of the instructions, and to ensure common information the monitor read the instructions aloud as the subjects followed along. The instructions noted that in each of the five periods each buyer would be paired with a different seller. In every period, the buyer determines an integer value (denoted  $p$  for price) to send to the seller, and requests a specific quality of the good (denoted  $q_r$  for quality request). Only the seller who is paired with the buyer is aware of these two choices. After the buyer makes these private decisions on the decision sheet, the monitor collects the sheets and walks them to the seller partners. Sellers then choose a quality level (denoted  $q$  for quality chosen), with an associated cost of quality that is increasing monotonically with product quality (denoted  $c(q)$ —see Appendix A for the cost of product quality parameters). The product quality choice is revealed only to the buyer partner; and, following the literature, all choices are revealed to the monitor.

Individual  $p$  and  $q$  choices combine to determine monetary payoffs for the pair according to the following payoff functions:

$$\text{Seller payoff: } \Pi_s = p - c(q) \quad (1)$$

$$\text{Buyer payoff: } \Pi_b = (v - p)q \quad v = \$80, p \in [\$5, \$80], q \in [.1, 1]$$

All payoff information was common information, and before beginning the experiment several hypothetical exercises were completed to ensure that everyone understood the instructions and payoff functions. Subjects were also aware that one of the five periods would be selected randomly

and that that particular period would determine payoffs. After the fifth period, subjects were paid in private after they completed the survey contained in Appendix B.

These parameter values yield a standard prediction under the assumption of common knowledge, self-interest theory, and appropriate backward induction. Since product quality is costly, sellers will choose the minimum level ( $q_{\min} = 0.1$ ). A buyer's best response is to choose  $p_{\min}$ , which is  $p = \$5$ . Thus, the subgame perfect equilibrium outcome is  $q^* = 0.1$  and  $p^* = 5$ , with associated profits of  $\Pi_s = \$5$  and  $\Pi_b = \$7.5$ , much less than more efficient profit levels (i.e.,  $p = 30$  and  $q = 0.5$  yields  $\Pi_s = \$24$  and  $\Pi_b = \$25$ ).

Previous experimental efforts have found that typically  $q > q^*$  and  $p > p^*$  and that  $\partial q / \partial p > 0$  in a reduced-form regression model, leading authors to conclude that reciprocity is important in economic interactions. The reciprocity inference is generally traced to Rabin's (1993) model of reciprocity (Fehr et al., 1997, p. 839), which describes a person with positive reciprocal motives as someone who responds to acts that are perceived as kind in a kind manner, even though there is no future pecuniary gain tied to this action. For the purposes herein, the literature has taken the qualitative implications of Rabin's (1993) model as meaning that the probability of nonshirking is increasing in the level of the perceived generosity of the offer. How generous an action is perceived is a difficult question to answer, however, and surely quite heterogeneous across agents, inducing the literature to operationalize reciprocity as meaning that  $\partial q / \partial p > 0$  in a reduced-form regression model (see, e.g., Fehr et al., 1993; Gächter and Falk, 2002).

Moving to column 2 in Table 1, Treatment Lab-RF (RF denotes replication with field values) simply manipulates the environment in Treatment Lab-R by setting

$$\text{Seller payoff:} \quad \Pi_s = p - c(q) \quad (2)$$

$$\text{Buyer payoff:} \quad \Pi_b = v(q) - p \quad p \in [\$5, \$80], q \in [1, 5].^7$$

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<sup>7</sup> The payoff function for the buyer is now similar to Fehr et al.'s (1997) S13-S16 treatments. In this case, now the price

For  $c(q)$  values, I use  $c(q) = \$4, \$5, \$8, \$15$ , and  $\$50$  for  $q = 1, 2, 3, 4, 5$ , and for  $v(q)$  values I use  $\$6, \$8, \$15, \$30$ , and  $\$80$  for  $q = 1, 2, 3, 4$ , and  $5$  (PSA 6, 7, 8, 9, or 10).<sup>8</sup> While these chosen values are admittedly only a rough estimate of the gains to trade available in this market, use of these parameters provides the necessary tension between the dominant strategy and the joint-profit maximization actions. Under this design, the Nash purely selfish prediction is  $p^* = \$5$ , and for sellers to send minimal card quality,  $q^* = 1$ . These actions result in  $\Pi_s = \$1$  and  $\Pi_b = \$1$ . The efficient quality level is  $q = 5$ , which ensures a joint surplus of  $\$30$ . Note that there could be losses of up to  $\$74$  (buyer sends  $\$80$  and receives the lowest quality Thomas card); as in the other induced value laboratory treatments herein—Treatments Lab and Lab-Context—after these treatments were carried out I had subjects participate in other unrelated experiments that did not involve interaction to ensure that they would leave with positive cash balances.

Treatment Lab-RF1 (RF1 denotes replication with field values in a purely one-shot setting) is identical to Treatment Lab-RF in every manner except that it is not executed over five periods with five different partners; rather it is a one-shot game. Since in the above treatments, by design subjects should have construed the setting as one-shot, Treatment Lab-RF and Treatment Lab-RF1 should yield similar data patterns if (i) subjects interpret Treatment Lab-RF as several one-shot games and (ii) experience does not unduly influence play. In total, Treatment Lab yields 77 data points for buyers and 77 data points for sellers in the gift exchange game.

Moving to row 2 in Table 1, Treatment Lab-Context adds context to Treatment Lab-RF1. In this case, rather than buyers and sellers transacting with abstract commodities, Treatment Lab-Context adds context that resembles the subjects' naturally occurring environment. For example, buyers make an offer to a seller to buy one 1990 *Leaf* Frank Thomas card and the buyer requests a

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represents a pure lump-sum transfer, which differs from the earlier joint profit equation which was characterized by price increases leading to an increase in the sum of payoffs when  $q < 1$ .

<sup>8</sup> Please see Appendix C for a discussion of how these values were obtained.

certain PSA grade. Similar to Treatment Lab-RF1, sellers have five PSA grades available (PSA 6, 7, 8, 9 or 10) and subsequently choose the quality of the Frank Thomas baseball card to give the buyer if they accept the buyer's offer.<sup>9</sup> Treatment Lab-Context includes 32 buyers and 32 sellers.

Completing the laboratory treatments is Treatment Lab-Market (more specifically, Treatment Lab-Market(\$20) and Treatment Lab-Market(\$65)). Treatment Lab-Market is the laboratory market parallel to Treatment Lab-Context: buying agents approach dealers in the experimental market to purchase 1990 *Leaf* Frank Thomas baseball cards in face-to-face transactions. Each participant's experience in Treatment Lab-Market followed four steps: (1) consideration of the invitation to participate in an experiment, (2) learning the market rules, (3) actual market participation, and (4) conclusion of the experiment and exit interview.

In Step 1, potential subjects approached the monitor's dealer table on the floor of the sportscard show and inquired about purchasing late 1980s/early 1990s baseball cards displayed on the table. If the subject was a white male roughly 25 years in age, the monitor asked if he was interested in participating in an experiment that would last about 30 minutes.<sup>10</sup> If the agent agreed to participate, the administrator explained that at a pre-specified time the subject should enter an adjacent room to take part in the experiment. Directions to the room were provided and the subject was informed that he would receive \$20 to participate in the experiment. To gather the dealer subject pool, I visited numerous dealer's tables and examined whether the dealer had a fair number (more than 5) of Thomas ungraded 1990 *Leaf* cards for sale that were of sufficiently heterogeneous quality. If the dealer had a sufficient number, he was asked if he would like to participate in a

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<sup>9</sup> PSA grades 6-10 were chosen because little trading of Thomas cards below PSA 6 is carried out in the actual market. Note that in this treatment I am not actually having agents transact with real commodities, rather subjects are told to act as if they are using graded Thomas cards.

<sup>10</sup> Given the results in List (2004a), I wished to avoid any confounds associated with statistical discrimination in this marketplace; hence I opted to use "majority" subjects as my buying agents in all treatments. This design choice may well give social preferences their best chance since the data in List (2004a) suggest that these buying agent types receive the best offers from dealers. Note, however, that any agent who desired to participate in an experiment was able to do so since the minority agents were asked to participate in an unrelated pilot experiment.

market experiment in which he could *potentially* sell some of the Thomas cards. Directions to the room and the appropriate times to enter the room were then provided to those dealers who agreed to participate. No show-up fee was given to dealers.

Upon arrival to the experimental market, in Step 2 a monitor thoroughly explained the market rules to subjects privately (buyers in one room and sellers in another). Consumers were informed that they would be “buyers” of 1990 *Leaf* Frank Thomas baseball cards in the experiment. The agents were told that they (typically in groups of 5) would enter the market and approach a pre-specified dealer, who had his Thomas cards displayed on his table in the experimental market. Importantly, in the spirit of the literature that suggests contracted negotiations can crowd out reciprocity (see, e.g., Fehr and List, 2004), I was careful to instruct buying agents to avoid haggling, while keeping the transaction as natural as possible.<sup>11</sup> In practice, negotiations are typically quite short or do not occur at all in this market (see List, 2004a, Table II); thus, besides realism this approach gives social preferences their best shot, since buying agents are signaling a fair amount of trust in the dealer when purchasing non-graded sportscards without much detailed negotiations. To ensure that buying agents did not aggressively bargain, their payoffs were not tied to quality or price; rather, they were paid \$20 for approaching two dealers. And, to maintain consistency with Treatment Lab-Context and afford the dealers reasonable price offers, the buying agent offered \$20 (or \$65) and requested a 1990 *Leaf* Thomas card that would merit a PSA 9 (10) if graded.

These parameter values were guided by the empirical results in Treatment Lab-Context (discussed below), current sportscard market values, and what would be naturally demanded in this environment. First, since the average buying agent sent \$20 to dealers in Treatment Lab-Context and requested a PSA 9 Thomas card, Treatment Lab-Market(\$20) is the naturally occurring

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<sup>11</sup> See also Macaulay (1963), who reports that “detailed negotiated contracts can get in the way of creating good exchange relationships between business units,” and Sitkin and Roth (1993, p. 376), who assert that “legalistic remedies can erode the interpersonal foundations of a relationship they are intended to bolster because they replace reliance on an individual’s good will with objective, formal requirements.”

analogue. Treatment Lab-Market(\$65) used the same dealers who were visited in Treatment Lab-Market(\$20), and was identical in every sense except that in this case buying agents offered \$65 for the Thomas card and requested a PSA 10. I chose \$65 because it is roughly 33 percent greater than  $c(10) = \$50$ , matching the relationship of  $c(9) = \$15$  and the \$20 value chosen in Treatment Lab-Market(\$20). Second, use of lower quality card levels would have been unnatural since the bulk of demanded volume in the market is for higher end card types, such as PSA 9 and 10.

In Step 3, the buying subjects each approached one dealer in round 1. Each interaction lasted less than 3 minutes and resulted in the purchase of a Thomas *Leaf* sportscard. Upon completing the transaction, the buyer departed the experimental market and physically gave the monitor the Thomas card in an adjacent room. After all transactions in round 1 were completed, buyers received instruction on which dealer to approach in round 2. Dealers were not allowed to communicate during this time period. The buying agents then re-entered the experimental market and approached a different dealer for the final buying period. Every dealer was approached twice—once with an offer of \$20 and a request for a PSA 9 card, and once with an offer of \$65 and a request for a PSA 10 card. The ordering of the offers was random.

Step 4 concluded the experiment—after subjects completed a confidential survey (see Appendix B) they departed. In total, I observed the behavior of 30 dealers who were each approached by two different buying agents offering either \$20 or \$65 — thus I have a sample size of 60 in Treatment Lab-Market.

Following the received gift exchange literature, if social preferences play a role in this case, then the card's grade and the offer price should be positively correlated:  $\partial q / \partial p > 0$  in a reduced-form regression model. Once the buying agents had purchased each of the cards in these treatments, the last step was to have the cards professionally graded. This was completed by having every card graded by a PSA representative.

Treatment Floor moves the exploration out of the laboratory and into the marketplace where these agents actually consummate business: the floor of the sportscard show.<sup>12</sup> Treatments Floor-\$20 and Floor-\$65 represent the naturally occurring analogues to Treatment Lab-Market and are identical whenever possible. Again, the buying agent's experience typically followed four steps. In Step 1, white males roughly 25-years old who were interested in late 1980s/early 1990s baseball cards were asked to participate in an experiment. If the agent agreed to participate, in Step 2 a monitor thoroughly explained the experimental rules. The agent was informed that he would be a "buyer" of 1990 *Leaf* Frank Thomas baseball cards in the experiment.

The agent was told that he would approach ten different dealers on the floor of a sportscard show to purchase the Thomas card. I was able to pre-select the dealers to be approached before the show by visiting their dealer table and examining whether they had more than 5 Thomas ungraded 1990 *Leaf* cards for sale that were of sufficiently heterogeneous quality. It is common practice for dealers to mill around the show looking at others' goods, and I was merely behaving in accordance with this norm when visiting dealer tables. Similar to Treatment Lab-Market, I was careful to instruct buying agents to avoid haggling, while keeping the transactions as natural as possible. And, the buying agent offered \$20 (or \$65) and requested a 1990 *Leaf* Thomas card that would merit a PSA 9 (10) if graded. In this spirit, much like Treatment Lab-Market, buying agents represented sophisticated buyers in that they expressed interest in a PSA graded card.

In Step 3, the subject approached dealers one at a time. Similar to Treatment Lab-Market, each interaction lasted less than 3 minutes and resulted in the purchase of a Thomas *Leaf* sportscard.

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<sup>12</sup> As I have noted elsewhere (e.g., List, 2004b, 2004c), with the rise in popularity of collector sportscards and memorabilia over the past two decades, markets that organize buyers and sellers have naturally arisen. Temporal assignment of the physical marketplace is typically done by a professional association or local sportscard dealer, who rents a large space, such as a gymnasium or hotel conference center, and allocates six-foot tables to dealers for a nominal fee. When the market opens, consumers mill around the marketplace, haggling and bargaining with dealers, who have their merchandise prominently displayed on their six-foot table. The duration of a typical sportscard show is a weekend, and subjects enter the market ready to buy, sell, and trade.

It should be noted that throughout the experiment the sportscard dealers were not aware that an experiment was occurring. This ensured that the process was as natural as possible for the dealers, whose behavior was of primary interest in this field experiment. Step 4 concluded the experiment—after subjects completed a confidential survey, they were paid \$20 in private.

A few noteworthy design issues should be mentioned before proceeding. First, each dealer was approached twice: once in Treatment Floor-\$20 and once in Treatment Floor-\$65. The spacing of visits was such to attenuate any suspicion—one example is that dealer  $i$  was approached by agent  $n$  on Friday night and by agent  $m$  on Sunday morning. And, the ordering of the visits was random—some dealers were approached in the \$20 treatment first, others were approached in the \$20 treatment second. I observed no ordering effect, so I suppress further discussion of this issue.

Second, unlike audit studies that test for market discrimination, I am directing the agent to buy the good. In this sense, these are not transactors who obliquely discontinue bargaining if the dealer accepts an offer; these are actual transactions. And, since transactions are typically in cash at sportscard shows, I provided the necessary funds to purchase the cards. Third, note that great care was taken to ensure that the data were gathered from interactions that would naturally occur in the marketplace. Subjects were entering the market to buy goods that were very similar to the good that I had them buying. Fourth, Treatments Lab-Market and Floor were carried out at several different sportscard shows in the same region in the U.S., from October 2002 to July 2004.

In total, I observe the behavior of 50 dealers who were each visited by two different agents (one in Treatment Floor-\$20 and one in Treatment Floor-\$65) — thus I have a sample size of 100 in Treatment Floor. Similar to Treatment Lab-Market, the last step of the experiment was to have the cards professionally graded. In addition, I should note that in every case I was able to obtain important subject-specific information from the dealers, either via a survey they completed during



an experiment in which they later participated or through filling out a survey (see Appendix B) in exchange for a payment of \$1.

To explore a level deeper into the underlying structure that organizes behavior in this market and control for potential selection effects, I complete three final treatments making use of natural exogeneity that the market offered during the sample period: while a third-party (PSA) has graded sportscards since 1987, no service existed prior to June of 2003 to grade sporting event tickets and ticket stubs. PSA announced their grading intentions in April 2003, but they did not provide grading criteria until June 2003. As noted earlier, Brown et al. (2004) highlight the attractiveness of such natural variation by arguing that such exogeneity is impossible to find in field data. I believe that these three field experimental treatments offer this useful characteristic.

Treatment Floor-NoGrading (denotes no grading available) is identical to Treatment Floor in that buyers approached dealers on the floor of a sportscard show (from October 2002 to March 2003) with either \$10 or \$30 to purchase an unused ticket or ticket stub. Given the thinness of the ticket market, it was necessary to use five different ticket types in the purchasing tasks (Cal Ripken's last game at Camden Yards, Cal Ripken's final game of "The Streak," Cal Ripken's "consecutive world-record breaking" game, and two World Series games). I was careful to choose tickets that were in the same price range to increase the likelihood of having the luxury of pooling the data. In total, I observe the behavior of 30 dealers in this treatment, and therefore gather 60 data points since each dealer is approached twice.

Treatment Floor-AnnounceGrading (denotes after announcement of grading) was completed at sportscard shows after PSA announced they would begin grading ticket stubs (April 2003) but before they released their grading scheme (June 2003). In this treatment, I purchased the same tickets and used the same protocol as in Treatment Floor-NoGrading. As outlined in row 4 column 2 in Table 1, I observe 54 dealer decisions in this treatment.

Completing the experimental design is Treatment Floor-Grading (denotes grading available), which is identical to Treatments Floor-NoGrading and Floor-AnnounceGrading, but was completed post-June 2003, a time period when grading services of tickets and ticket stubs existed. I observe 36 total dealer decisions in this final treatment. Accordingly, I purchased 150 tickets in these three treatments; and similar to Treatment Floor, I subsequently had every ticket graded by a PSA representative. In Appendix C, I provide the necessary institutional details about the sportscard and sports ticket grading industry to motivate the experimental design.

### ***Identification and Hypotheses***

The sportscard marketplace includes both local and non-local sellers (dealers). Accordingly, by parsing dealers into types, my experimental design permits two distinct identification strategies. First, with the maintained assumption that non-local dealers have no reputational concerns, any reciprocal behavior observed among this lot of subjects in the Floor treatments can be attributable to social preferences. Alternatively, behavior of local dealers might include reputational as well as social preference effects. Second, even if I observe disparate data patterns across local and non-local dealers, it might be the case that selection effects are at work. Indeed, one of the key points of the theoretical work on social preferences is that there is a distribution of types in the population—some purely selfish types and others with social preferences. In this spirit, it is important to consider a second means of identification: behavior *within* dealer type—across the laboratory and field settings, as well as within the field treatments.

Under this design, a clean test of the predictions from a purely self-interested model and a model with social preferences is permitted. Table 2 provides a summary of what can be examined across the various experimental treatments. In a general sense, Table 2 highlights that laboratory experiments that estimate propensities might not provide the necessary environment to measure them accurately. Consider column 1 of Table 2, which summarizes predictions in the lab

treatments. In this case, I delineate between two potential situations: one that has “experimenter” effects and one that does not have “experimenter” effects. For example, in the lab treatments, reputational concerns (i.e., sellers are fully aware that the experimenter can document delivered quality), experimenter demand effects, Hawthorne effects, or simply that the task is undertaken in an artificial setting can each potentially confound any preferred interpretation.<sup>13</sup> Though the definition is much too narrow, for minimalism I denote such *potential* laboratory effects simply as “reputational” concerns in Table 2.

These effects are important to consider when delineating between predictions of the self-interest and social preference models. If such effects are present in the lab, then both models predict that there will be gift exchange, or a positive relationship between price and quality. Of course, in the field treatments (predictions contained in columns 2 and 3 of Table 2), by design, such effects are varied, permitting a clean test of the two models. As I progress through the results summary below, I will highlight whether evidence is consistent with each of the theories.

### III. Experimental Results

Table 3 provides a summary of the raw data. The table can be read as follows: Treatment Lab-R in row 1, column 1, denotes that the average price in this treatment was \$28.40, average quality was 3.5, and average requested quality was 6.1. Note that in Table 3, for comparability reasons, I have scaled Treatment Lab-R data to range from 1-10, and PSA 6, 7, 8, 9, and 10 are denoted as quality levels 1, 2, 3, 4, and 5.<sup>14</sup> A first result relates to the comparison between the behavior of this subject pool and students. As Fehr and List (2004) note, a typical criticism levied

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<sup>13</sup> In social psychology, several studies due to Martin Orne, Robert Rosenthal, and others discuss the important effects of the experimenter-subject relationship (see, e.g., Orne (1962) and Rosenthal’s (2002) summary). While efforts to expunge such effects have been explored in the experimental literature using double-blind (e.g., Hoffman et al., 1996), randomized response (e.g., List et al., 2004), and related techniques, such approaches may attenuate a number of laboratory phenomena, but seem incapable of completely eliminating them, and might even introduce other biases.

<sup>14</sup> Average individual payoffs (ranges) are as follows: Treatment Lab-R: buyers, \$14.90 (\$6.50 to \$24), sellers, \$18.60 (\$5 to \$34); Lab-RF: buyers, \$2.40 (-\$59 to \$25), sellers, \$8.00 (\$1 to \$61); Lab-RF1: buyers, \$0.22 (-\$25 to \$25), sellers, \$9.81 (\$1 to \$35); Lab-Context: buyers, -\$0.09 (-\$67 to \$25), sellers, \$8.44 (\$1 to \$70).

against experimental results concerns the fact that most economics experiments are conducted with students. This may be problematic for several reasons. For example, due to selection effects, those who do not behave like students may have selected into roles and be overrepresented in certain parts of the economy (e.g., sellers in the marketplace). The first result addresses this issue.

**Result 1:** *Behavior of sportscard enthusiasts in laboratory games is in line with the gift exchange literature using student subjects, and the results extend well to one-shot environments.*

Evidence for Result 1 is contained in the raw statistics in row 1 of Table 3, which are consistent with the raw data gathered in laboratory experiments with student subjects (see, e.g., Fehr et al, 1993; Charness, 1996). Figure 1 complements Table 3 by mapping the relationship between product quality and prices for Treatment Lab-R. Figures 2 and 3 provide similar insights, using data from Treatments Lab-RF and Lab-RF1. Overall, the trajectory of the data clearly shows that product quality and prices are positively related. In addition, when I examine the temporal aspect of the data there is little variation over time, consistent with previous studies on gift exchange (for an exception, see Charness et al., 2004).

To provide the necessary statistical link to the literature, I follow previous work and estimate Tobit random effects regression models. The dependent variable in the regressions is the quality of the good, which is regressed on the price transfer and controls for dealer-specific effects:

$$q_{it} = \beta p_{it} + \omega_{it}. \quad (3)$$

In equation (3),  $q_{it}$  represents the product quality that dealer  $i$  sent to the buyer in period  $t$ ;  $p_{it}$  denotes the buying agent's offer price to dealer  $i$  in period  $t$ ; and  $\omega_{it}$  includes a white noise error term with mean zero, and a constant in the Tobit model. This specification is augmented by inclusion of dealer random effects in the Tobit random effects regression model.<sup>15</sup>

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<sup>15</sup> Including a time trend does not change the qualitative results. And, when the models converged, controlling for buyer-specific effects does not change the qualitative empirical results presented below. In addition, I have explored empirically modeling the relationship between buyer and seller rents:  $(v-p) = f(p-c)$  in a regression framework. Similar insights as those presented below are obtained so I suppress further discussion.

Regression results presented in columns 1-3 of Table 4 provide evidence that dealers reward buyers for paying higher prices. In each of the three treatments the marginal price effect is positive and statistically significant at the  $p < .10$  level using a two-sided alternative. This result is consistent with the received gift exchange literature. When applicable, I also present an estimate of  $\theta$  in Table 4.  $\theta$  is equal to  $\partial v(q)/\partial P$  and provides a natural benchmark of gift exchange expressed in monetary units. In the case of Treatments Lab-RF and Lab-RF1,  $\theta$  estimates are both significantly different from zero, suggesting that gift exchange occurs at the margin. In terms of economic significance, a  $\theta$  estimate of 1.1 in Treatment Lab-RF1 suggests that a \$1 increase in  $P$  leads to a \$1.10 increase in reciprocated gift,  $v(q)$ .

While these results provide a robustness check of the data gathered in the laboratory with student subjects and represent good news in that the major laboratory results seem to spill over to different subject pools who are commonly engaged in similar exercises in their everyday lives, one can push the comparability notion a bit harder by adding field context to the laboratory environment. This approach is inherent in Treatment Lab-Context, which yields

**Result 2:** *Adding natural context influences behavior, but gift exchange remains alive and well.*

Evidence for this result can be found in the summary of Treatment Lab-Context data contained in Table 3. Treatment Lab-Context data reveal that average prices and quality levels are only slightly lower than what was observed in Treatment Lab-RF1 (the comparable context-free treatment). Slight behavioral differences are also revealed when comparing Figures 3 and 4, which show i) that the positive relationship remains in the contextual data, but that there is a slightly greater mass at the sub-game perfect equilibrium prediction: 13 of 32 (41%) observations in Treatment Lab-Context versus 9 of 27 (33%) observations in Treatment Lab-RF1, and ii) that there is a greater number of price (quality) realizations at \$25 (3) and below in Treatment Lab-Context.

Moving to the data from Treatment Lab-Market, Table 3 shows that the positive relationship between price and product quality is evident in the aggregate data: whereas the average quality was 3.1 (PSA 8.1) in Treatment Lab-Market(\$20), it was 4.1 (PSA 9.1) in Treatment Lab-Market(\$65). Figure 5 provides an ocular view of these quality differences. Comparing the proportion of sellers who provided various quality levels across the \$20 and \$65 treatment yields a discernible rightward shift in the distribution of Treatment Lab-Market(\$65) data.<sup>16</sup> In terms of the average monetary value of the return gift ( $v(q)$ ), sellers provided \$19.73 in Treatment Lab-Market(\$20) and \$41.33 in Treatment Lab-Market(\$65).

To compare gift exchange on the margin across these two treatments, I return to equation (3) and estimate a Tobit model. For Treatment Lab-Context data, the marginal price effect is positive and statistically significant at conventional levels—see column 4 in Table 4. It is interesting to note that the marginal effect estimate (0.05) is slightly lower than the marginal effect estimate in Treatment Lab-RF1 (0.07), and  $\theta$  is considerably lower: \$0.65 versus \$1.1. Upon pooling Treatment Lab-RF1 and Treatment Lab-Context data and estimating equation (3), however, a likelihood ratio test suggests that the homogeneity null should not be rejected, suggesting that behavioral differences do not exist across Treatments Lab-RF1 and Lab-Context.

Considering Treatment Lab-Market data, I provide marginal effects estimates from a Tobit random effects model in column 5 of Table 4. The marginal effect estimate of 0.02 is positive and significant at conventional levels—this estimate suggests that card quality increases by roughly 1 grade when the buyer offers \$65 rather than \$20; in this case,  $\theta$  is equal to \$0.45.<sup>17</sup> Accordingly,

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<sup>16</sup> In some instances dealers made quality claims, and these included statements that they could not provide the requested quality. Similar to Treatments Floor (cards) and Floor (tickets), I still had my buyers purchase the good in Treatment Lab-Market and provide this information to me in the survey (see Appendix B). I consider mendacious claims below.

<sup>17</sup> In addition to the Tobit random effects estimation strategy, which is heavily utilized in the literature, since there is a natural ordering in the data and there are only 5 cells (i.e., PSA 6-10), I supplement these results by using a panel data ordered probit model, as described in Appendix D. Empirical estimates from the panel data ordered probit model are suppressed because they always coincide with insights gained from equation (3).

the overall pattern of results suggests that gift exchange is alive and well, even when market context is utilized in the experimental design.

Results 1 and 2 provide a nice validity check of the extant gift exchange literature. Yet, as Table 2 highlights, this evidence does not unequivocally show that these subjects exhibit social preferences. It might be the case that these dealers are purely selfish and due to effects associated with the lab environment they behave in a manner consistent with gift exchange. A necessary next step is to explore behavior in naturally occurring environments where the controls of the experiment are relaxed appropriately. In such a setting, experimenter demand effects, Hawthorne effects, and the like are absent since, unbeknownst to them, experimental subjects (sellers) are randomly chosen from the dealers who have certain goods. A first insight from the field treatments is:

**Result 3:** *When third-party verification is available, behavior in naturally occurring transactions is consonant with both gift exchange and a concern for reputation.*

Tables 3 and 4 as well as Figure 6 provide evidence for Result 3. Row 3 in Table 3 shows that the positive relationship between price and product quality is evident in the aggregate data: whereas the average quality was 2.1 (PSA 7.1) in Treatment Floor-\$20, it was 3.2 (PSA 8.2) in Treatment Floor-\$65. In terms of the average monetary value of the return gift ( $v(q)$ ), however, sellers provided much less than they provided in Treatment Lab-Market: roughly \$8 in Treatment Floor-\$20 and \$20 in Treatment Floor-\$65 (versus \$19.73 and \$41.33 in Treatment Lab-Market). This difference is highlighted via a comparison of Figures 5 and 6, which reveals the significant leftward shift in the quality distributions across both the \$20 and \$65 price offers when moving from the lab to the field. Concerning the ticket stub data, I find that Treatments Floor-AnnounceGrading and Floor-Grading displayed in row 4 of Table 3 support the positive relationship found in the sportscard data.

Regression results in Table 4 yield similar insights: estimates in column 6 of Table 4 provide evidence that product quality and price are positively correlated in Treatment Floor (cards), as the marginal effect estimate of 0.02 is positive and significant at conventional levels. This

estimate, which is quite similar to the marginal effect in Treatment Lab-Market, suggests that card quality increases by roughly 1 grade when the buyer offers \$65 rather than \$20. In this case, however, since the quality change is from PSA 7 to PSA 8 (rather than PSA 8 to PSA 9 in Treatment Lab-Market),  $\theta$  is equal to \$0.21, considerably lower than the  $\theta$  estimate of 0.45 in Treatment Lab-Market. A similar result is found in the Treatment Floor-AnnounceGrading and Treatment Floor-Grading data presented in columns 8 and 9 of Table 4, although the marginal price effect is not statistically significant in the Treatment Floor-Grading data at conventional levels. Upon pooling the Treatment Floor-AnnounceGrading and Treatment Floor-Grading data (a likelihood ratio test indicates pooling is appropriate:  $\chi^2 = 5.8$ ), however, the marginal price effect, contained in the rightmost column of Table 4, is statistically significant. Interestingly, across all three specifications the marginal price effect estimate is 0.02, and  $\theta$  is approximately \$0.20.<sup>18</sup>

As Table 2 shows, considering that this data pattern is observationally equivalent to predictions from a model based purely on reputational effects (e.g., Klein and Leffler, 1981), again, these insights are not unequivocal evidence in favor of gift exchange. One can explore a level deeper by recognizing that some of the dealers in the sample may have had an economic reason to uphold their reputations, whereas others may not have had similar incentives. A next result follows:

**Result 4:** *When third-party verification is possible, local dealer behavior in naturally occurring transactions is consonant with both gift exchange and a concern for reputation, whereas non-local dealers' behavior is in line with self-interest theory.*

Table 5 and Figures 6a, 6b, 7, and 8 provide evidence for this result. In splitting the dealer types, a dealer is labeled as a “non-local” if he or she is unlikely to be concerned with reputation effects—for example, if he or she rarely attends sportscard shows in the area (fewer than three times in a typical year), does not plan to attend more frequently than this in the future, does not own a sportscard shop, and does not have an Internet sportscard business. All other dealers are labeled as

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<sup>18</sup> When computing  $\theta$  in the ticket specifications,  $v(q)$  is equivalent to one-half the value of  $v(q)$  in the sportscard data.



“locals”—in practice, these are primarily dealers who frequent the area often. This information was obtained from the survey in Appendix B. Note that besides this difference, across all other observables, such as years of experience and age, dealers are similar. I return to the issue of selection effects below, however.<sup>19</sup>

The raw data displayed in Figures 6a, 6b, 7, and 8 provide initial support for Result 4. When transacting with local dealers, higher price offers yield superior quality in Treatments Floor, Floor-AnnounceGrading, and Floor-Grading, as illustrated in Figures 6a and 7. Alternatively, while delivered quality is positively related to price across these three treatments among non-local dealers (see Figures 6b and 8), the differences are minute.

Table 5 provides regression results to support Result 4. Columns 1 and 2 split the Treatment Floor data into two subsamples: Floor(cards)<sub>L</sub> (local dealers over sportscards) and Floor(cards)<sub>N</sub> (non-local dealers over sportscards). In the former subsample, the marginal price effect is positive and significant at conventional levels. In terms of economic significance, the coefficient estimate in column 1 of 0.03 results in an estimated marginal effect of roughly 1.5 grades: that is, in the \$65 treatment local dealers provided a quality that was 1.5 grades above the quality level they provided in the \$20 treatment. Measured at the sample means, this 1.5 quality increment yields the buyer a PSA rated 8.6 card rather than a PSA rated 7.1 card. Using the  $v(q)$  values discussed earlier, this quality increase maps into an increase in market value of roughly \$20, much less than the extra \$45 spent to obtain the card. A  $\theta$  estimate of \$0.31 complements this finding.

Alternatively, for non-local dealers gift exchange is not evident in Treatment Floor (see column 2 of Table 5), as the marginal price effect is not statistically significant at conventional levels. Regression results for Treatments Floor-AnnounceGrading and Floor-Grading provide

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<sup>19</sup> When I categorize Treatment Lab and Treatment Lab-Market dealer data in a similar manner I find that there is no difference in behavior across local and non-local dealers.

further support for Result 4: in both cases the marginal price effect in the local dealer data is positive and significant at conventional levels (columns 5 and 7 of Table 5), whereas there is no such effect found in the non-local dealer data (columns 6 and 8 of Table 5). For both the Treatment Floor-AnnounceGrading and Floor-Grading local dealer data, the marginal effect estimate is 0.04, and  $\theta$  is \$0.32 and \$0.42, though neither  $\theta$  estimate is statistically significant at conventional levels. Upon pooling these data (LLR test:  $\chi^2 = 1.4$ ),  $\theta$  equals \$0.35 and is significant at the  $p < .05$  level (rightmost column of Table 5). Treating non-local dealer data similarly by pooling Treatment Floor- AnnounceGrading<sub>N</sub> and Floor-Grading<sub>N</sub> data provides little new information: gift exchange is not evident among non-local dealers. Accordingly, as Table 2 suggests, the non-local dealer data are consonant with the self-interest model.

A natural question that arises concerns whether the local dealer behavior is driven primarily by reputation effects or social preferences—given the identification problem, from the above results alone one cannot determine the extent to which reputation effects and social preferences are influencing market outcomes. One nice characteristic of the current experimental design is that I can examine behavior in markets that are void of third-party verification to explore this issue. In such cases, in economic terms the situation faced by the local and non-local dealers is identical. Treatment Floor-NoGrading provides a first result:

**Result 5:** *When third-party verification is not available, supply side behavior in naturally occurring transactions is consonant with purely selfish money-maximizing theory, suggesting that reputational considerations, rather than social preferences, are driving the earlier results.*

Evidence for this result can be seen in Tables 3-5 as well as Figures 7 and 8. Table 3 shows that there is very little quality difference between the \$10 and \$30 offers in Treatment Floor-NoGrading. This result is highlighted in Figures 7 and 8, where both local and non-local dealers do not provide different quality levels across offers of \$10 and \$30 in Treatment Floor-NoGrading. Empirical results displayed in Tables 4 and 5 support the raw data patterns, as the marginal price effect is

insignificant in the aggregate data (column 7 in Table 4) and in both specifications that split the data by dealer type (columns 3 and 4 in Table 5).

This finding, which according to the predictions outlined in Table 2 is in line with the self-interest model, leads to the tentative conclusion that reputation effects rather than social preferences are responsible for driving a large part of the price/quality tendencies observed in the naturally occurring data. While there is some evidence in favor of social preferences in this market, as price and quality are directionally related (positively) in various places in the non-local dealer data and in the local dealer Treatment Floor-NoGrading data, it seems to be of second-order importance in real market transactions.<sup>20</sup> This insight can be viewed in Appendix E, which provides several supplementary figures that summarize the raw data across local and non-local dealers in the ticket stub treatments.

### ***Mendacious Claims***

Empirical estimates presented above provide measures of gift exchange in the spirit of the extant literature and highlight a framework that can measure social preference effects and reputation effects. Yet it is important to recognize and examine the degree of mendacious claims in the marketplace. If dealers do not have the necessary inventory to fulfill the quality request (for example, due to my misjudgment of quality during my perusal or due to sales during the show) but provide quality disclaimers, then it is important to explore this aspect of behavior. In this spirit, an important complement to the above results is a thorough analysis of the statistical association between quality claimed and quality delivered. A first result follows:

**Result 6:** *When third-party verification is possible, local dealers provide fewer claims of quality than non-local dealers in the field, and conditional on claiming quality, shirk less frequently.*

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<sup>20</sup> I also gathered information on length of buyer/seller relationships. While speculative, an upper bound estimate of social preferences within long term relationships (where a long term relationship is defined as one wherein the buyer and dealer have had five or more interactions in the previous 12 months or have had two or more interactions annually over the past 3+ years) suggests that they influence the price and quality correlation.

Table 6 summarizes dealer behavior across Treatments Floor (cards) and Floor (ticket stubs). Evidence for the first part of Result 6 can be obtained by computing the percentage of local and non-local dealers who claim quality in Treatments Floor, Floor-AnnounceGrading, and Floor-Grading. The second part of Result 6 follows from a comparison of the quality claimed and the quality actually delivered. Before discussing the evidence for Result 6, it is important to point out that in some cases dealers provide quality ranges – for example, “this card would grade at PSA 8 or 9.” In these cases I use the mid-point of the range (e.g., 8.5). A few other dealers were agnostic about the grading system—I label these types as not claiming quality (similar results are obtained if I simply delete these observations). And, in some instances the dealer stated “this one is top quality” or “this is a gem” when describing the good. I label these dealers as not claiming quality, but should note that if I take the literal word of the dealer and pair these statements with the appropriate PSA grade the fundamental results do not change.

Upon pooling the Treatment Floor, Floor-AnnounceGrading, and Floor-Grading data in Table 6, I find that 94 of 190 (49%) dealer observations involve product quality claims. Split by dealer type, 38 of 120 (32%) local dealer observations involve product quality claims, whereas 56 of 70 (80%) non-local dealer observations involve product quality claims. Of those dealers who make quality claims, local dealers deliver the promised quality (or above) in 18 of 38 cases (47%), whereas non-locals deliver the promised quality (or above) in only 5 of 57 (9%) cases.

To complement these insights, I estimate the bivariate probit model with sample selection due to van de Ven and van Praag (1981):

$$Y_1^* = \Phi(\beta_1'V) + \varepsilon_1; \quad Y_1 = 1 \text{ if } Y_1^* > 0, 0 \text{ otherwise} \quad (4a)$$

$$Y_2^* = \Omega(\beta_2'Z) + \varepsilon_2; \quad Y_2 = 1 \text{ if } Y_2^* > 0, 0 \text{ otherwise}; \quad (4b)$$

$$\varepsilon_1, \varepsilon_2 \sim \text{bivariate normal}(0, 0, 1, 1, \rho).$$

Equation (4a) is the quality claim equation.  $Y_1^*$  is unobserved, but I can observe its sign since  $Y_1$  equals 1 if the dealer provided a quality claim, 0 otherwise. Variables in  $V$  include a dichotomous

variable indicating whether the seller is a local dealer, and a control for the price offer/quality requested. Equation (4b) is the shirking equation and is observed only when  $Y_1 = 1$ ; hence the selectivity model arises since a mendacious claim can only occur among dealers who provide a quality promise. In  $Z$ , I include a dichotomous variable for whether the seller is a local dealer. To account for data dependencies, I calculate the standard errors assuming the error terms are independent across dealers but not within each dealer (i.e., clustered standard errors for dealers).

The individual quality claim and shirking equations could be estimated separately, but unless  $\rho = 0$  such estimation results in sample selection bias. In estimation of the system, I use full information maximum likelihood, where the log-likelihood is given by:

$$\sum_{y_1, y_2=1} \ln \phi_2(\beta_1' V, \beta_2' Z, \rho) + \sum_{y_1=1, y_2=0} \ln \phi_2(\beta_1' V, -\beta_2' Z, -\rho) + \sum_{y_1=0} \ln \phi(-\beta_1' V). \quad (5)$$

$\phi_2$  denotes the bivariate standard normal cumulative density function and  $\phi$  denotes the univariate standard normal cumulative density function.

Empirical results in support of Result 6 are presented in columns 1, 2, and 5-8 in Table 7. The first part of Result 6—when third-party verification is possible, in the field local dealers provide fewer quality claims—can be found in all three quality claim equations.<sup>21</sup> For instance, empirical estimates in column 1 suggest that in the sportscard treatment local dealers provide fewer quality claims, and this estimate is statistically significant at the  $p < .05$  level. As is clear from columns 5 and 7 in Table 7, similar insights are obtained in the ticket treatments. Evidence in favor of the second part of Result 6—conditional on claiming quality, when third-party verification is possible local dealers shirk less often in the field—can also be found in all three shirking specifications. Whereas the coefficient estimates in Treatment Floor (cards) and Floor-Grading specifications are only significant at the  $p < .07$  ( $p < .17$ ) level using a two-sided alternative

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<sup>21</sup> The “Floor-Grading” model failed to converge; thus I present estimates from equations that are estimated separately.

(columns 2 and 8), the coefficient estimate in the Floor Announce-Grading model is statistically significant at the  $p < .01$  level (column 6).

Similar to the spirit of the inquiry into Result 4, one can question whether the increased quality promises and deliveries from local dealers are due purely to reputational concerns or have an element of social preferences. Examining data collected in Treatment Lab-Market and Treatment Floor (ticket stubs) lends insights into this issue and leads to the final result:

**Result 7:** *In the laboratory, or when third-party verification is not possible, local and non-local dealers make similar claims of quality, and conditional on claiming quality, shirk to the same extent.*

As Table 6 reveals, in Treatments Lab-Market and Floor-NoGrading, local and non-local dealers behave quite similarly. In Treatment Floor-NoGrading, local dealers make quality claims in 22 of 36 (61%) cases, whereas non-local dealers make quality claims in 14 of 24 (58%) cases. Likewise, conditional on claiming quality, local dealers in Treatment Floor-NoGrading shirk in 18 of 22 cases—i.e., in 82% of transactions local dealers provide lower quality than promised—whereas 71% (10 of 14) of non-local dealer transactions should be considered shirking. In Treatment Lab-Market, both dealer types make considerably fewer quality claims, and never shirk.

Estimating the bivariate probit model with sample selection in equations (4a) and (4b), I find that the observed differences across local and non-local dealers are statistically insignificant at conventional levels for both Treatment Lab-Market and Treatment Floor-NoGrading data. For Treatment Floor-NoGrading, columns 3 and 4 of Table 7 show that both local and non-local dealers make similar quality claims and conditional on providing a quality claim shirk to the same extent. For Treatment Lab-Market data, column 9 in Table 7 provides similar evidence: local and non-local dealers provide a similar number of quality claims.<sup>22</sup>

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<sup>22</sup> As Table 6 reveals, the shirking equation cannot be estimated for the Treatment Lab-Market data because no dealers—local or non-local—shirked in the laboratory experiment.

#### IV. Concluding Remarks

This study provides a framework for measuring social preferences and reputation effects using a series of laboratory and field experiments. In doing so, it showcases the desirability of building a bridge between the lab and the field. In a methodological sense, this bridge permits a test of whether laboratory behavior is a good indicator of behavior in the field. The finding that agents behave differently in tightly controlled laboratory experiments than in their naturally occurring environment poses an important challenge to laboratory studies that measure individual propensities. More generally, these results underscore the role that field experiments can play in empirical economics. For example, experimentalists typically take stock in results from a series of *laboratory* experiments. This study pushes this notion in a new direction by shedding light on the importance of including results from field experiments within this evidentiary system. In this light, the results show that field experiments can help to uncover the causes and underlying conditions necessary to produce data patterns observed in the lab.

While the data suggest that social preferences do not have a major impact in these particular markets, such results, of course, do not necessarily preclude social preferences from having import within other economic domains. Some scholars have argued that such preferences are evident in domains where the pressures of the market are absent (e.g., the charitable fundraising work of Falk, 2004). I view this class of studies as fruitful in that they represent good examples of domains where such preferences might be significant.

The data are also sufficiently rich to speak to how reputation effects and professional certification influence market performance. For example, the data support the view that reputation effects enhance the quality of goods. This insight is consonant with Akerlof (1970), who provides evidence on the operation of markets in developing countries that demonstrates a positive association between reputation effects and the quality of goods. Furthermore, empirical results

suggest that third-party enforcement of contracts is important: the addition of professional quality certifiers enhances market performance and supports the conjecture that the private market can solve the lemons problem through third party verification. This result might be viewed as a test of the Klein and Leffler (1981) model in that local sellers cheat less than non-local sellers when quality is measurable, but this is not the case when quality is not easily measurable. This finding indicates that reputations cannot work without information, suggesting that reputation and the monitoring of quality are *complements*.



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**Table 1. Experimental Design**

<b>Treatment Lab</b>	<b>Treatment Lab-R</b> <i>Replicate lab studies</i>  $n = 25$	<b>Treatment Lab-RF</b> <i>Extend to field values</i>  $n = 25$	<b>Treatment Lab-RF1</b> <i>Extend to one-shot environment</i>  $n = 27$
<b>Treatment Lab-Context</b>	<b>Treatment Lab-Context</b> <i>Adds market context</i> $n = 32$	<b>Treatment Lab-Market(\$20)</b> <i>Adds market interaction</i> $n = 30$	<b>Treatment Lab-Market(\$65)</b> <i>Adds market interaction</i> $n = 30$
<b>Treatment Floor (cards)</b>	<b>Treatment Floor-\$20</b> <i>Naturally occurring sportscard market</i> $n = 50$	<b>Treatment Floor-\$65</b> <i>Naturally occurring sportscard market</i> $n = 50$	
<b>Treatment Floor (tickets)</b>	<b>Treatment Floor-NoGrading</b> <i>Naturally occurring ticket market before grading was available</i> $n = 60$	<b>Treatment Floor-AnnounceGrading</b> <i>Naturally occurring ticket market post-grading announcement</i> $n = 54$	<b>Treatment Floor-Grading</b> <i>Naturally occurring ticket market when grading service was available</i> $n = 36$

Notes: Each cell represents one (or two, in the case of Treatment Floor (tickets)) unique treatment. For example, Treatment Lab-R in row 1, column 1, denotes that 25 dealer and 25 nondealer observations were gathered to replicate the laboratory gift exchange studies in the literature.

**Table 2. Predictions: Self-Interested Model versus Social Preference Model**

<i>Lab Treatments</i>	<i>Floor Treatments Sportscards</i>	<i>Floor Treatments Tickets</i>
<p><b>Self-interested model:</b>  <i>Without experimenter effects:</i>            No relationship between price and quality</p> <p><i>With experimenter effects:</i>            Positive relationship between price and quality due to, for example, reputation effects</p>	<p><b>Self-interested model:</b>  <i>Local dealers:</i> Positive relationship between price and quality due to, for example, reputation effects</p> <p><i>Non-local dealers:</i> No relationship between price and quality</p>	<p><u><i>Floor-NoGrading</i></u>  <b>Self-interested model:</b>  <i>Local dealers:</i> No relationship between price and quality</p> <p><i>Non-local dealers:</i> No relationship between price and quality</p>
<p><b>Social preference model:</b>  <i>Without experimenter effects:</i>            Positive relationship between price and quality due to social preferences</p> <p><i>With experimenter effects:</i>            Positive relationship between price and quality due to, for example, reputation effects and social preferences</p>	<p><b>Social preference model:</b>  <i>Local dealers:</i> Positive relationship between price and quality due to, for example, reputation effects and social preferences</p> <p><i>Non-local dealers:</i> Positive relationship between price and quality due to social preferences</p>	<p><b>Social preference model:</b>  <i>Local dealers:</i> Positive relationship between price and quality due to social preferences</p> <p><i>Non-local dealers:</i> Positive relationship between price and quality due to social preferences</p>
		<p><u><i>Floor-AnnounceGrading and Floor-Grading</i></u>  <b>Self-interested model:</b>  <i>Local dealers:</i> Positive relationship between price and quality due to, for example, reputation effects</p> <p><i>Non-local dealers:</i> No relationship between price and quality</p>
		<p><b>Social preference model:</b>  <i>Local dealers:</i> Positive relationship between price and quality due to, for example, reputation effects and social preferences</p> <p><i>Non-local dealers:</i> Positive relationship between price and quality due to social preferences</p>

Notes: Each column represents predictions of the self-interested model versus the social preference model across the three major experimental types. In splitting the dealer types, a dealer is labeled as a “non-local” if he or she is unlikely to be concerned with reputation effects—for example, if he or she rarely attends sportscard shows in the area (fewer than three times in a typical year), does not plan to attend more frequently than this in the future, does not own a sportscard shop, and does not have an Internet sportscard business. All other dealers are labeled as “locals”. When I split Treatment Lab and Treatment Lab-Market data in a similar manner I find that there is no difference in behavior across local and non-local dealers.

**Table 3. Results Summary**

<b>Treatment Lab</b>	<b>Treatment Lab-R</b> p = 28.4(16.1) q = 3.5(2.0) q <sub>r</sub> = 6.1(2.1)	<b>Treatment Lab-RF</b> p = 22.6(20.7) q = 2.3(1.4) q <sub>r</sub> = 4.1(0.9)	<b>Treatment Lab-RF1</b> p = 24.8(22.1) q = 2.5(1.7) q <sub>r</sub> = 4.0(1.3)
<b>Treatment Lab-Context</b>	<b>Treatment Lab-Context</b> p = 19.5(19.6) q = 2.3(1.5) q <sub>r</sub> = 4.2(1.1)	<b>Treatment Lab-Market(\$20)</b> p = \$20 q = 3.1 (0.9) q <sub>r</sub> = 4	<b>Treatment Lab-Market(\$65)</b> p = \$65 q = 4.1 (0.6) q <sub>r</sub> = 5
<b>Treatment Floor (cards)</b>	<b>Treatment Floor-\$20</b> p = \$20 q = 2.1(0.9) q <sub>r</sub> = 4	<b>Treatment Floor-\$65</b> p = \$65 q = 3.2(1.0) q <sub>r</sub> = 5	
<b>Treatment Floor (tickets)</b>	<b>Treatment Floor-NoGrading</b> p = \$10      p = \$30 q = 2.7(0.6)   q = 2.7(0.7) q <sub>r</sub> = 4      q <sub>r</sub> = 5	<b>Treatment Floor-AnnounceGrading</b> p = \$10      p = \$30 q = 2.9(0.6)   q = 3.4(0.8) q <sub>r</sub> = 4      q <sub>r</sub> = 5	<b>Treatment Floor-Grading</b> p = \$10      p = \$30 q = 3.1(0.8)   q = 3.6(1.1) q <sub>r</sub> = 4      q <sub>r</sub> = 5

Notes: Each cell represents summary statistics from one (or two in the case of Treatment Floor (tickets)) unique treatment. For example, Treatment I-R in row 1, column 1, denotes that the average price in this treatment was \$28.40, average quality was 3.5, and average requested quality was 6.1. Treatment I-R data are scaled to range from 1-10, and PSA 6, 7, 8, 9, and 10 are denoted as quality levels 1, 2, 3, 4, and 5 in the table. Standard deviations are in parentheses.

**Table 4: Marginal Effects Estimates for the Sellers' Provided Quality<sup>a,b</sup>**

Variable	<i>Treatment Type</i>									
	Lab-R	Lab-RF	Lab-RF1	Lab-Context	Lab-Market	Floor(cards)	Floor-No Grading	Floor-Announce Grading	Floor-Grading	Floor-Pooled
<i>Price</i>	0.05* (1.8)	0.05^ (3.3)	0.07^ (4.3)	0.05^ (4.3)	0.02^ (4.4)	0.02^ (6.6)	-0.001 (0.01)	0.02^ (2.1)	0.02 (1.1)	0.02^ (2.6)
<i>Constant</i>	0.6 (0.7)	-0.4 (0.7)	-0.9^ (3.3)	-0.8^ (2.9)	1.6^ (6.2)	0.6^ (3.1)	1.7^ (8.0)	1.7^ (5.8)	1.8^ (3.3)	1.7^ (7.3)
$\theta$	---	\$0.72^ (3.6)	\$1.1^ (6.9)	\$0.65^ (4.7)	0.45^ (2.1)	\$0.21^ (5.0)	\$0.01 (0.3)	\$0.17 (1.1)	\$0.23 (1.1)	\$0.19^ (2.3)
Dealer Random Effects	YES	YES	NO	NO	YES	YES	YES	YES	YES	YES
N	25	25	27	32	60	100	60	54	36	90

<sup>a</sup>Dependent variable is the sellers' product quality given to the buyer. Floor-pooled pools Floor-AnnounceGrading and Floor-Grading data.  $\theta$  is the monetary gift exchange estimate, computed as  $\partial v(q)/\partial P$ .

<sup>b</sup>t-ratios (in absolute value) are beneath marginal effect estimates.

^ Significant at the .05 level; \*Significant at the .10 level.

**Table 5: Marginal Effects Estimates for the Sellers' Provided Quality Split by Dealer Type<sup>a,b,c</sup>**

Variable	<i>Treatment Type</i>									
	Floor(cards) <sub>L</sub>	Floor(cards) <sub>N</sub>	Floor-No Grading <sub>L</sub>	Floor-No Grading <sub>N</sub>	Floor-Announce Grading <sub>L</sub>	Floor-Announce Grading <sub>N</sub>	Floor-Grade <sub>L</sub>	Floor-Grade <sub>N</sub>	Floor-Pool <sub>L</sub>	
<i>Price</i>	0.03^ (8.6)	0.004 (0.7)	0.002 (0.2)	-0.005 (0.5)	0.04^ (2.1)	0.003 (0.3)	0.04^ (2.7)	0.003 (0.1)	0.04^ (4.8)	
<i>Constant</i>	0.6^ (4.1)	0.6^ (4.6)	1.6^ (5.0)	1.8^ (5.2)	1.7^ (5.2)	1.5^ (4.6)	1.8^ (5.0)	1.8* (1.7)	1.8^ (10.0)	
$\theta$	\$0.31^ (5.2)	\$0.01 (0.5)	\$0.02 (0.4)	-\$0.006 (0.5)	\$0.32 (1.4)	\$0.02 (0.6)	\$0.42 (1.5)	\$0.03 (0.1)	\$0.35^ (2.1)	
Dealer Random Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	
N	70	30	36	24	30	24	20	16	50	

<sup>a</sup>Dependent variable is the sellers' product quality given to the buyer. Floor-pool<sub>L</sub> pools Floor-AnnounceGrading<sub>L</sub> and Floor-Grading<sub>L</sub> data.  $\theta$  is computed as  $\partial v(q)/\partial P$ .

<sup>b</sup>t-ratios (in absolute value) are beneath marginal effect estimates.

<sup>c</sup>"L" ("N") after treatment type denotes regression with "local" ("non-local") dealer data only.

^ Significant at the .05 level; \*Significant at the .10 level.

**Table 6. Results Summary—Product Quality Claims**

	<b>Treatment Lab-Market</b>	<b>Treatment Floor (cards)</b>	<b>Treatment Floor-NoGrading</b>	<b>Treatment Floor-AnnounceGrading</b>	<b>Treatment Floor-Grading</b>
<b>Overall</b>	Claims: 10/60 Quality claim: 3.95(0.4) Delivered quality: 4.3(0.5) Delivered promised quality or above: 10/10	Claims: 53/100 Quality claim: 3.9(0.7) Delivered quality: 2.7(1.1) Delivered promised quality or above: 15/53	Claims: 36/60 Quality claim: 3.8(0.6) Delivered quality: 2.8(0.6) Delivered promised quality or above: 8/36	Claims: 24/54 Quality claim: 4.2(0.5) Delivered quality: 2.9(0.9) Delivered promised quality or above: 4/25	Claims: 17/36 Quality claim: 4.2(0.6) Delivered quality: 3.1(1.1) Delivered promised quality or above: 4/17
<b>Local dealers</b>	Claims: 6/42 Quality claim: 4.0(0.5) Delivered quality: 4.3(0.5) Delivered promised quality or above: 6/6	Claims: 27/70 Quality claim: 3.9(0.7) Delivered quality: 3.4(1.1) Delivered promised quality or above: 12/27	Claims: 22/36 Quality claim: 3.9(0.5) Delivered quality: 2.8(0.6) Delivered promised quality or above: 4/22	Claims: 7/30 Quality claim: 4.1(0.3) Delivered quality: 3.9(0.4) Delivered promised quality or above: 4/7	Claims: 4/20 Quality claim: 4.3(1.0) Delivered quality: 3.8(0.5) Delivered promised quality or above: 2/4
<b>Non-local dealers</b>	Claims: 4/18 Quality claim: 3.9(0.3) Delivered quality: 4.3(0.5) Delivered promised quality or above: 4/4	Claims: 26/30 Quality claim: 4.0(0.7) Delivered quality: 2.0(0.6) Delivered promised quality or above: 3/26	Claims: 14/24 Quality claim: 3.7(0.6) Delivered quality: 2.8(0.6) Delivered promised quality or above: 4/14	Claims: 17/24 Quality claim: 4.3(0.6) Delivered quality: 2.5(0.6) Delivered promised quality or above: 0/18	Claims: 13/16 Quality claim: 4.2(0.4) Delivered quality: 2.9(1.2) Delivered promised quality or above: 2/13

Notes: Each cell represents summary statistics from one unique treatment. For example, row 1, column 2, denotes that in the Treatment Floor data, 53 of 100 dealer observations involved a claim of product quality. The average claim was 3.9 (PSA 8.9) and the average delivered product quality was 2.7 (PSA 7.7). Standard deviations are in parentheses besides means.

**Table 7: Empirical Estimates for the Sellers' Quality Claims and Shirking Rates<sup>a,b,c</sup>**

Variable	<i>Treatment Type</i>								
	Floor (cards)		Floor (tickets) No Grading		Floor (tickets) Announce-Grading		Floor- (tickets) Grading		Lab- Market
	Quality Claim	Shirk	Quality Claim	Shirk	Quality Claim	Shirk	Quality Claim	Shirk	Quality Claim
<i>Local Dealer</i>	-1.4 <sup>^</sup> (4.4)	-1.2* (1.8)	0.06 (0.2)	0.24 (0.6)	-1.3 <sup>^</sup> (2.5)	-2.3 <sup>^</sup> (4.8)	-2.2 <sup>^</sup> (2.2)	-1.0 (1.4)	-0.30 (0.8)
<i>Price</i>	0.01* (1.9)	---	-0.03* (1.7)	---	-0.01 (0.3)	---	0.04 (1.3)	---	-0.01 (0.7)
<i>Constant</i>	0.6 <sup>^</sup> (2.0)	1.1 <sup>^</sup> (2.4)	0.7* (1.7)	-0.1 (0.3)	0.6 (1.5)	1.0 <sup>^</sup> (2.8)	0.3 (0.5)	1.0 <sup>^</sup> (2.4)	-0.5 (0.5)
N	100	53	60	36	54	24	36	17	60

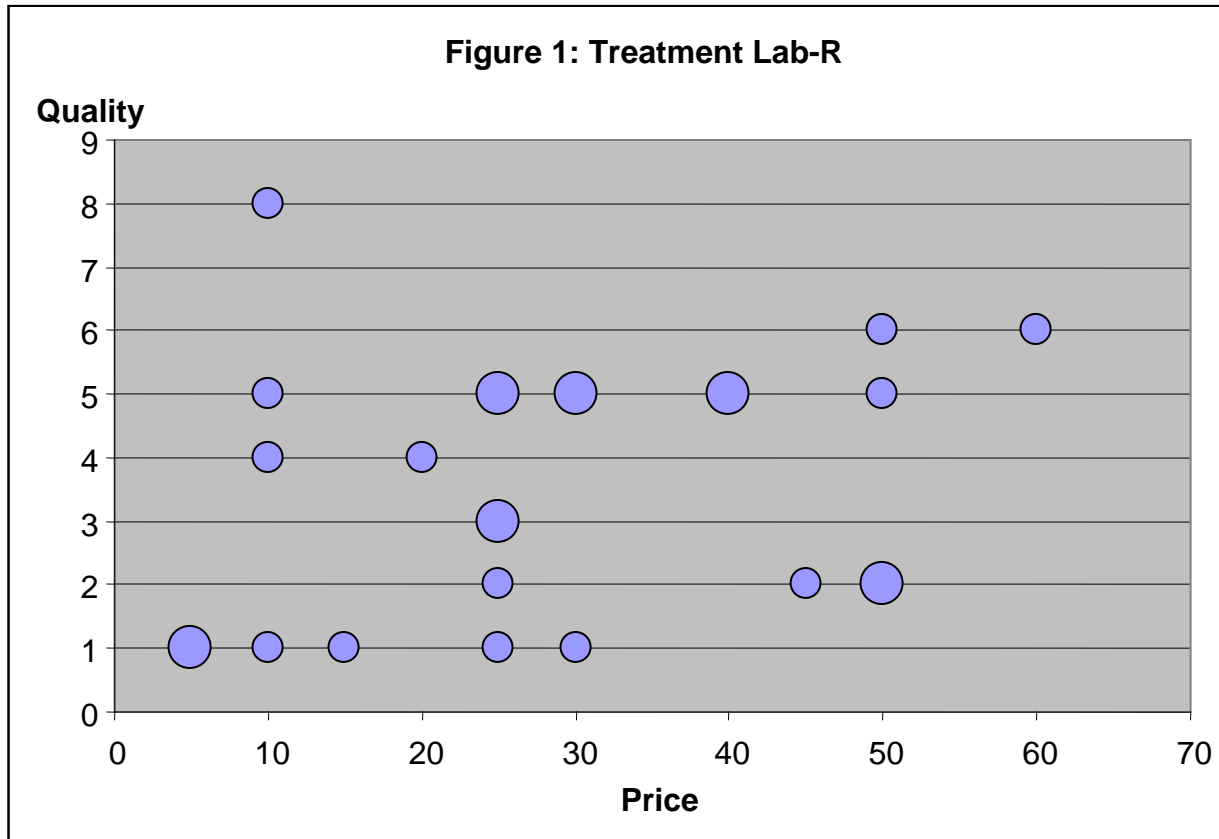
<sup>a</sup>Dependent variable in the Quality Claim specification equals one if the dealer claimed quality, 0 otherwise. Dependent variable in the Shirking specification equals one if the dealer shirked on the quality claim, 0 otherwise.

<sup>b</sup>The “Floor-Grading” model did not converge; thus estimates are derived from equations that are estimated separately. The second stage shirking equation for Lab-Market cannot be estimated because no dealers shirked in the Lab-Market treatment.

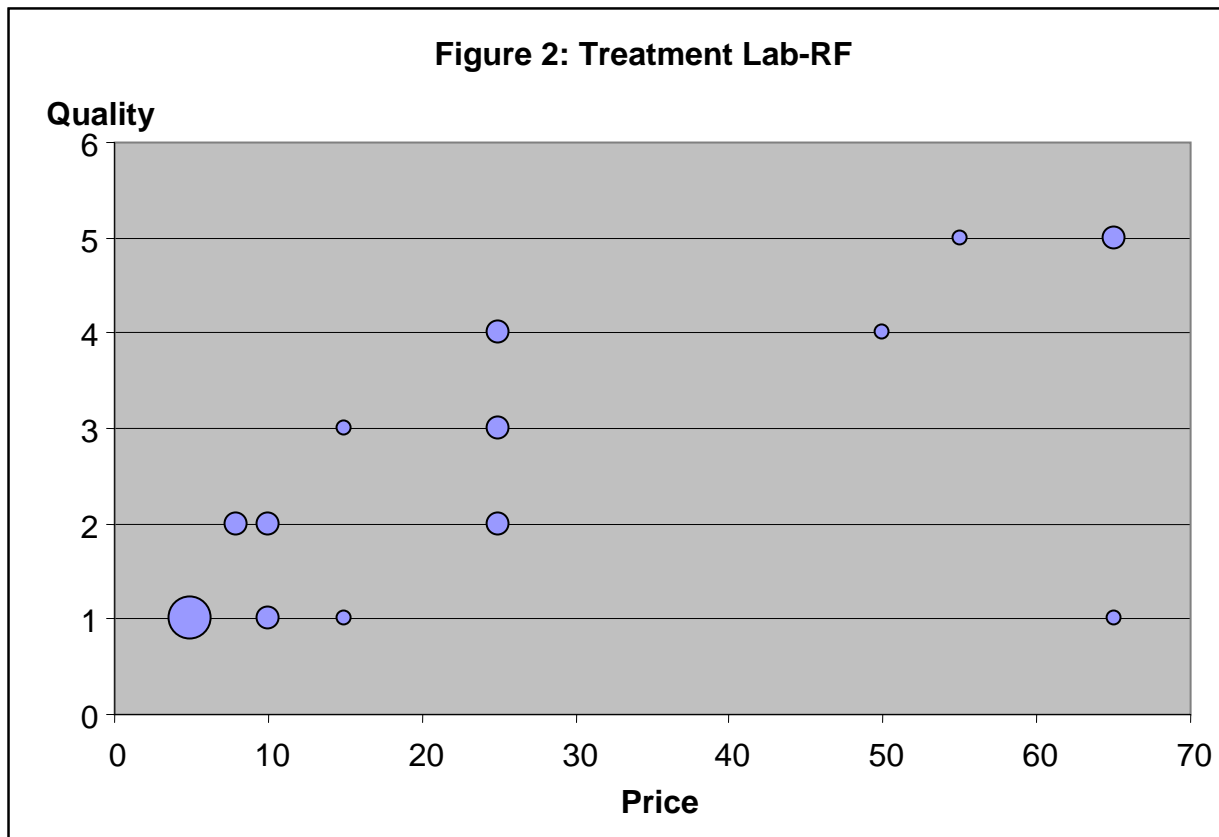
<sup>c</sup>t-ratios (in absolute value) are beneath coefficient estimates.

<sup>^</sup> Significant at the .05 level; \*Significant at the .10 level.

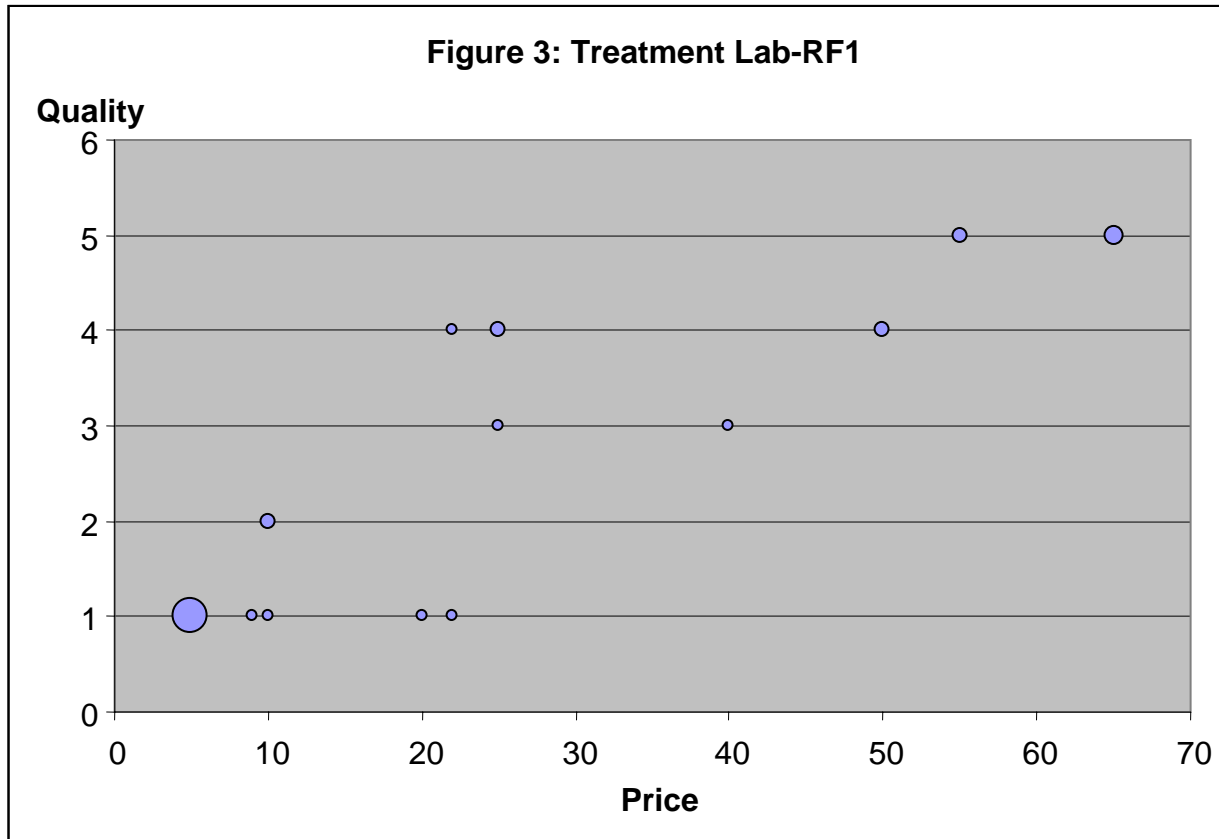




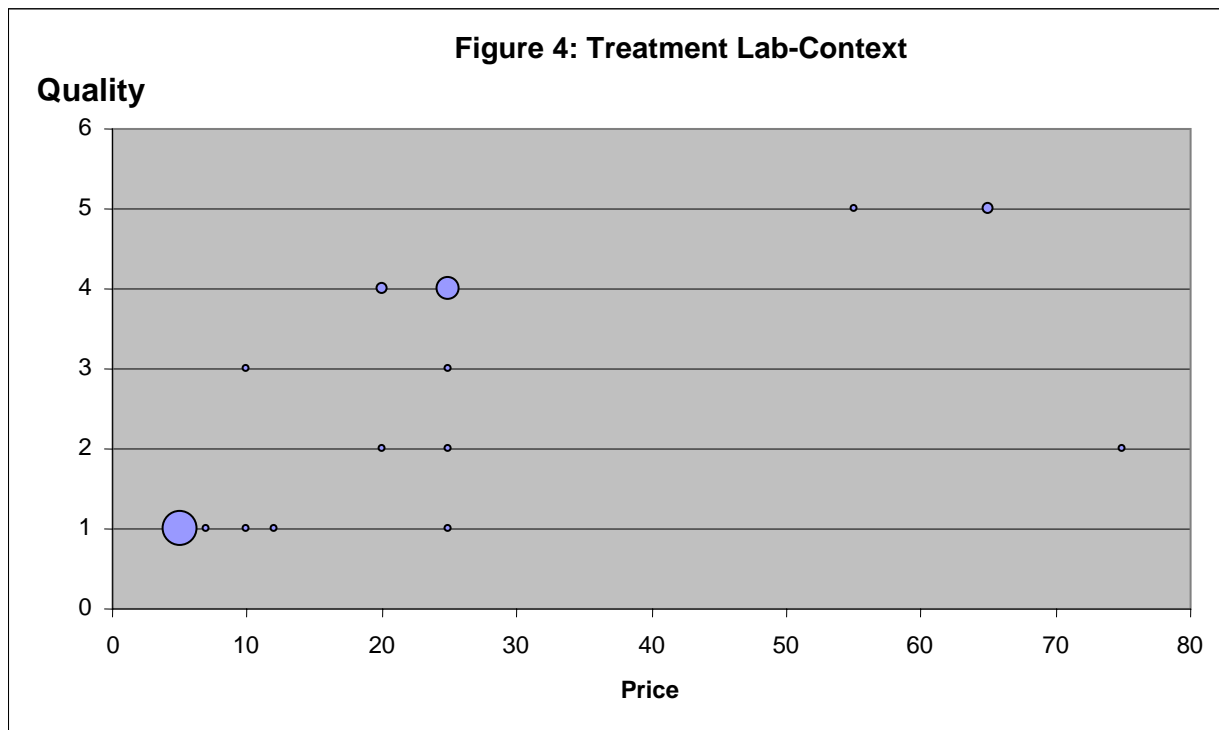
Note: Larger-sized circles indicate that a greater number of observations occur at that point.



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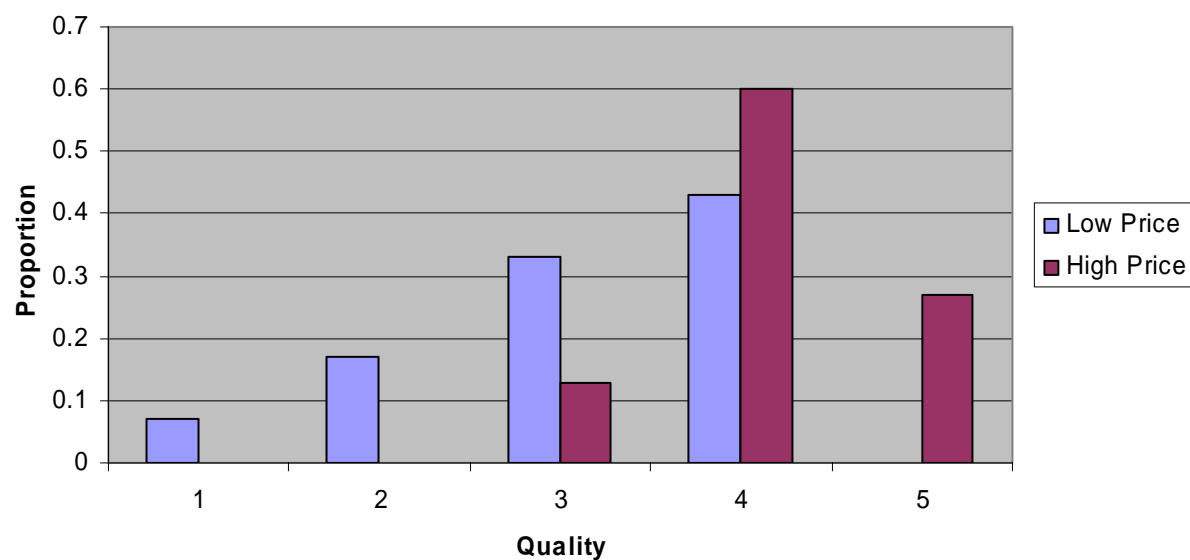


Note: Larger-sized circles indicate that a greater number of observations occur at that point.

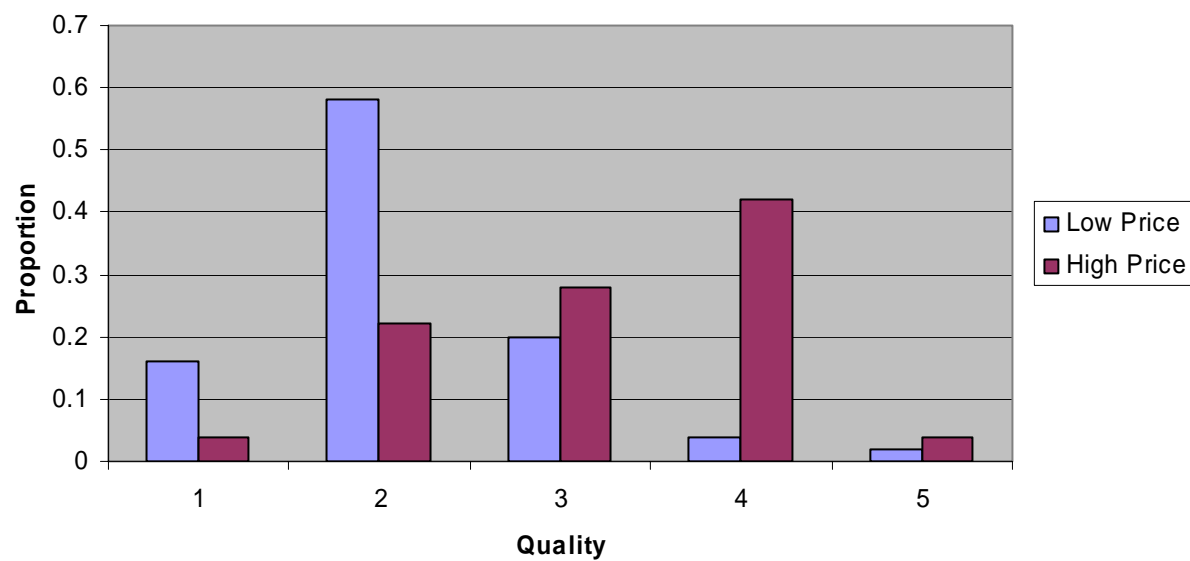


Note: Larger-sized circles indicate that a greater number of observations occur at that point.

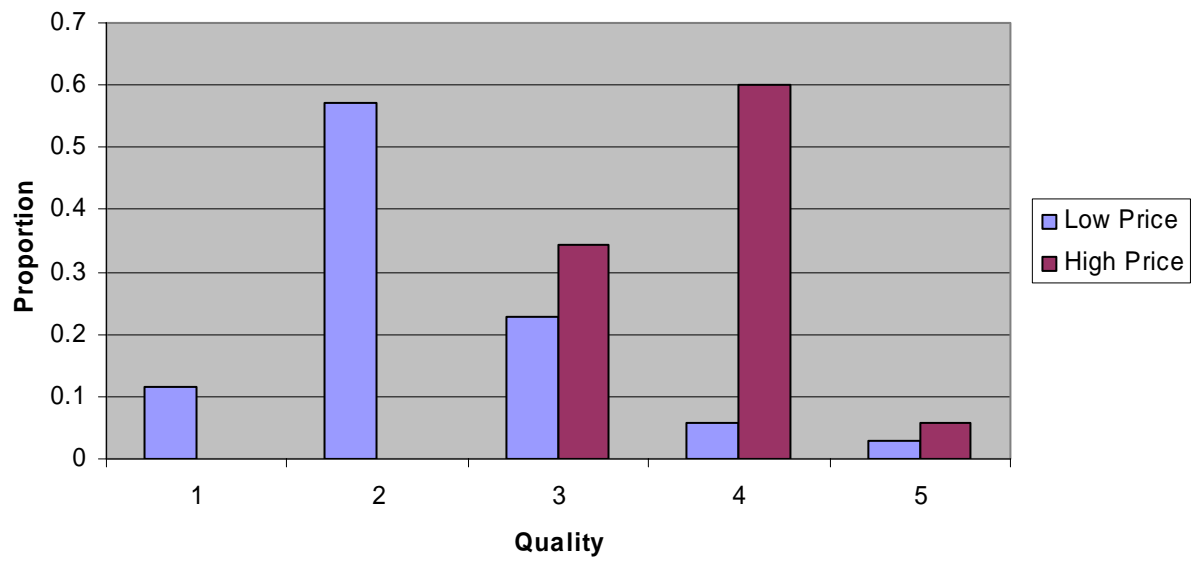
**Figure 5: Treatment Lab-Market**



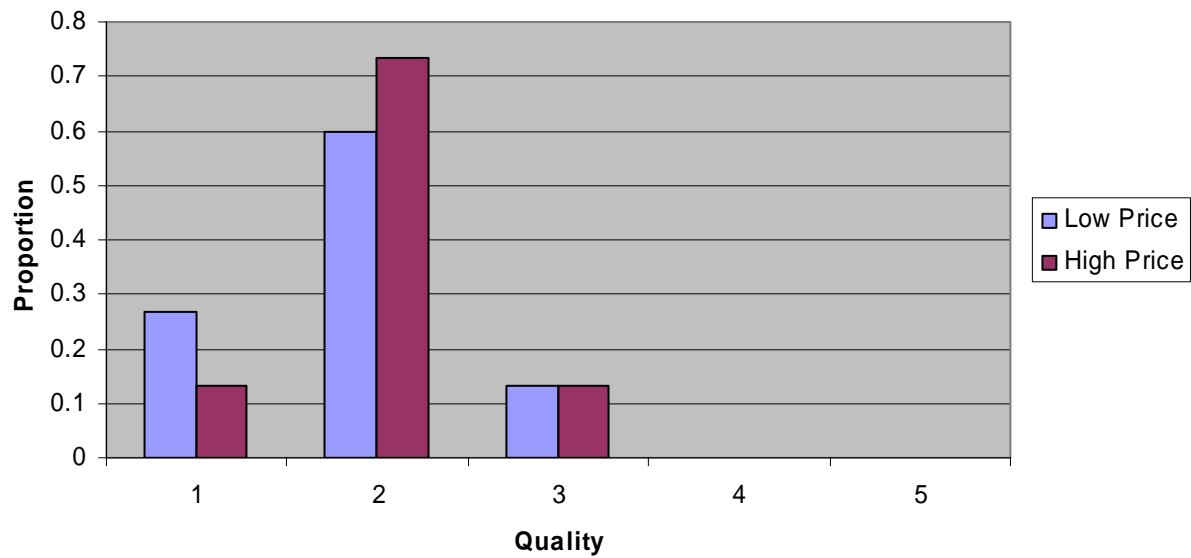
**Figure 6: Treatment Floor (sportscards)**



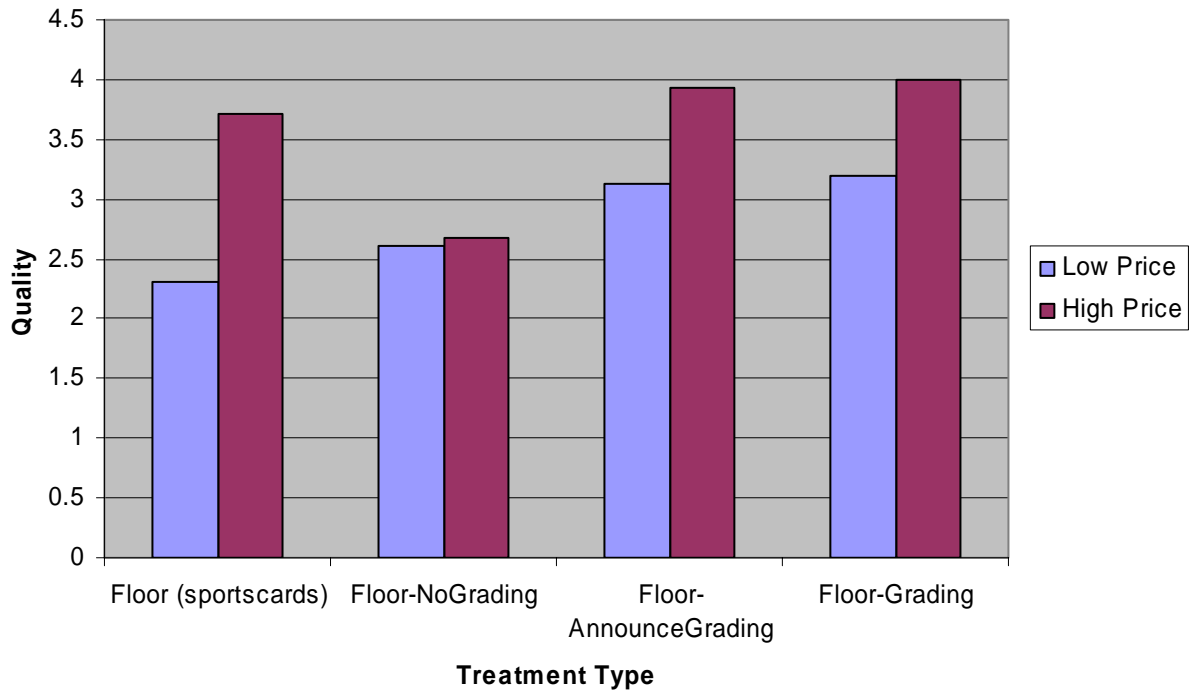
**Figure 6a: Treatment Floor-Local Dealers**



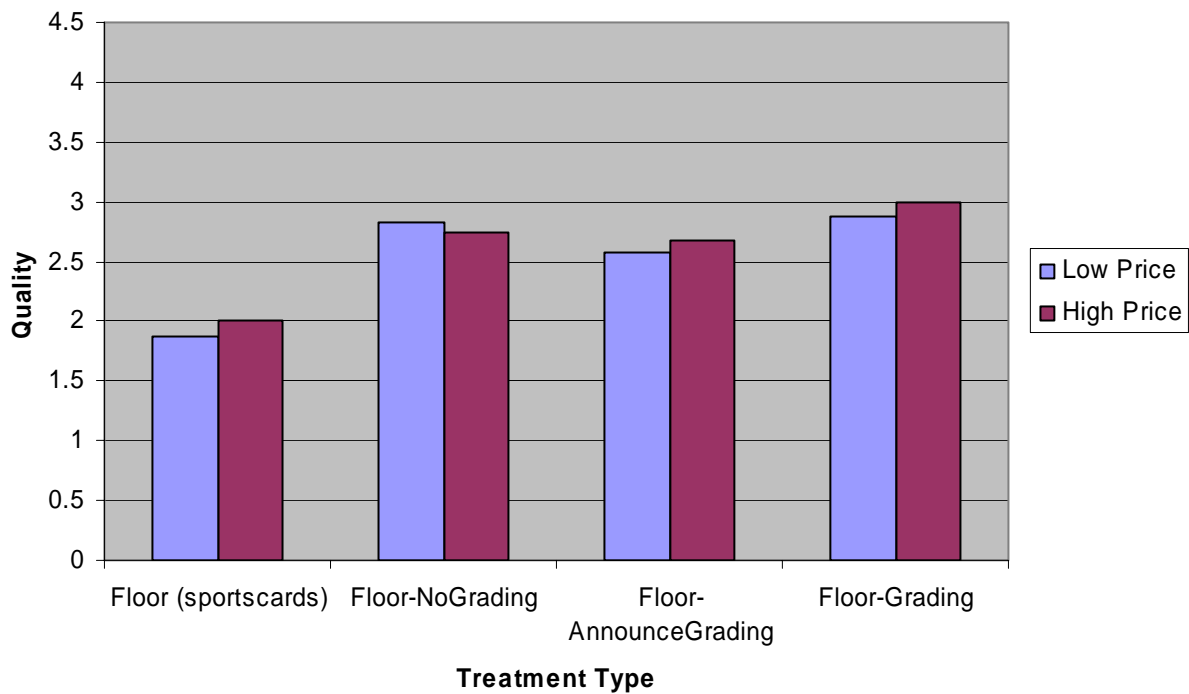
**Figure 6b: Treatment Floor-Non-Local Dealers**



**Figure 7: Price/Quality Relationship for Local Dealers**



**Figure 8: Price/Quality Relationship for Non-Local Dealers**



## Appendix A. Summary Experimental Instructions: Treatment Lab-R

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Welcome to an economics experiment. The instructions are simple, and if you read them carefully and make appropriate decisions, you can earn a considerable amount of money. All of the profits you make in this experiment and other subsequent experiments carried out today will be summed and paid to you in cash in private.

This experiment consists of two stages:

1. 5 non-dealers are buyers, 5 dealers are sellers. You will notice the room divider that separates buyers from sellers—please do not attempt to see who is on the other side of the divider. In the first stage, a buyer will make an offer to a seller to buy one unit of a fictitious good. The buyer will also request a certain product quality.
2. In the second stage, the seller decides whether to accept the price offer and decides on the product quality.

Some important pieces of information:

- A. This same decision problem will take place five times (or five periods). Note, however, that buyers will be paired with a different seller each time, thus you are never paired with the same person twice. Earnings will be computed by randomly selecting one of the five periods for payment—the chosen period will be carried out for cash.
- B. Sellers cannot make counteroffers—they merely decide whether or not to accept the offer and the product quality.
- C. Profit of a buyer is the difference between \$80 and the price at which he has bought the good. This difference is then multiplied by the quality of the good chosen by the seller. Thus, the formula to compute buyer profits is:

$$(\$80 - \text{price paid}) * \text{quality}$$

- D. If buyers decide to make an offer to buy the good from the seller, they must choose a whole number between (or including) \$5 and \$80.
- E. The buyer writes down this price offer and a requested quality on the decision sheet provided. Do not announce your decision publicly.
- F. After writing these choices on the sheet, a monitor will take the sheet to a seller, who views the choice and decides whether or not to accept the offer. If he accepts the offer, he then determines product quality, which must be between (or including) 0.1 and 1. Sellers are not required to provide the quality that the buyer requested. Sellers should write down their choices on the sheet (whether to accept or not, and product quality), after which the sheets are returned to buyers to compute profits.
- G. Seller's profits are given by:

$$\text{Price paid by the buyer} - \text{cost of providing the good}$$

If sellers do not accept the offer, both the buyer and seller receive \$0 for that period. Seller's costs depend on their choice of product quality, as follows:

Quality	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1.0
Cost	0	\$1	\$2	\$4	\$6	\$8	\$10	\$12	\$15	\$18

The table shows that the highest quality good (quality = 1) costs the dealer \$18 to provide. A quality of 0.1 costs the dealer \$0. For buyers, if the chosen quality is 1.0, then their profits are simply \$80 – price paid. Otherwise, this difference (\$80 – price paid) is multiplied by a fraction less than 1.

Are there any questions? Let's now go over a few practice problems to ensure everyone understands the rules and how to compute payoffs.

## **Appendix B. Confidential Survey Summary**

These questions will be used for statistical purposes only. THIS INFORMATION WILL BE KEPT STRICTLY CONFIDENTIAL AND WILL BE DESTROYED UPON COMPLETION OF THE STUDY.

1. How long have you been active in the sportscard and memorabilia market? \_\_\_\_\_yrs

1a. How often do you have your sportscards professionally graded?

Always

Sometimes

Rarely

Never

2. Are you a sportscard or sports memorabilia professional dealer? \_\_\_\_\_

2a. If yes, in a typical month, how often do you visit this area as a sportscard dealer? \_\_\_\_\_

2b. As a dealer, how often do you plan to set up in this area in the coming months?  
\_\_\_\_\_

2c. Do you have an Internet sportscard/memorabilia business? \_\_\_\_\_

2d. Do you sell on *eBay* or on a different Internet site using your dealer name? \_\_\_\_\_

2e. If yes, how often do you sell? \_\_\_\_\_

2f. Do you own a sportscard shop? \_\_\_\_\_

3. Gender: 1) Male 2) Female

4. Age \_\_\_\_\_ Date of Birth \_\_\_\_\_

### **Additional questions for Treatments Lab-Market and Lab-Floor (cards and tickets)**

5. Have you had previous interactions with that dealer? \_\_\_\_\_ If yes, how many? \_\_\_\_\_ Over how many years? \_\_\_\_\_.

6. Did the dealer provide any “guarantee” about the PSA grade of the card [ticket]? For example, did the dealer state that “this card [ticket] would grade at PSA 9 [if such services were available]”? Please comment. \_\_\_\_\_  
\_\_\_\_\_

## Appendix C. Further Experimental and Institutional Details

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**A. Determining  $c(q)$  and  $v(q)$  values.**  $C(q)$  values were chosen to represent the dealer cost to replace a 1990 *Leaf* Frank Thomas card of various quality levels.<sup>23</sup> The values are taken from the standard price guide for baseball cards—*Beckett Baseball Cards Monthly*. For each single type of ungraded card, Beckett collects pricing information from about 110 card dealers throughout the country and publishes a “high” and “low” price reflecting current selling ranges for several quality variants. The high price represents the highest reported selling price and the low price represents the lowest price one could expect to find with extensive shopping. Assuming that dealers’ replacement costs are roughly equivalent to the reported “low” price, I use the “low” prices from Beckett for 1990 *Leaf* Thomas cards that would grade PSA 6, 7, 8, 9, and 10 to approximate  $c(q)$  values.

Determining  $v(q)$  values in equation (2) to approximate the gains from trade is more difficult since consumer demand curves are not readily observable. In this case I considered results from two approaches: i) taking the “high” prices from *Beckett* for 1990 *Leaf* Thomas cards that would grade PSA 6, 7, 8, 9, and 10, and ii) gathering statements of value for 1990 *Leaf* Thomas cards that would grade PSA 6, 7, 8, 9, and 10 via a contingent survey in the spirit of Cummings and Taylor (1999).<sup>24</sup> The contingent valuation experiment, which was run on the floor of a sportscard show, randomly allocated consumers into one of 5 treatments (PSA 6, 7, 8, 9, or 10). Thirty subjects were placed into each treatment, for a total of 150 subjects. Subjects were asked to state their true value for a 1990 *Leaf* Thomas card in a contingent valuation scenario. In addition, they were warned about hypothetical bias, which oftentimes arises in such situations, with a “cheap talk” script.<sup>25</sup> Previous efforts have found that a contingent survey that includes a cheap talk script has yielded consumer values that closely match actual values (e.g., List, 2001). Most importantly for our purposes, mean values from the contingent survey are in the range of published *Beckett* “high” prices; thus, I use these values and make  $v(q) = \$6, \$8, \$15, \$30, \text{ and } \$80$  for  $q = 1, 2, 3, 4, \text{ and } 5$  (PSA 6, 7, 8, 9, or 10).

**B. Sportscard and Sports Ticket Grading.** Each year, sportscard companies design and print sets of sportscards depicting players and events from the previous season. Once the print run of a particular set has been completed, the supply of each distinct card in the set is fixed. The value of a particular card depends on its scarcity, the player depicted, and the physical condition of the card—i.e., the condition of its edges, corners, and surface, and centering of the printing. To track card condition, people often use a 10-point scale. For example, a card with flawless characteristics under

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<sup>23</sup> I chose this particular card for all treatments because of my experience in evaluating the attributes of the card over the past 15 years (as a dealer and a consumer), Thomas’ popularity, and the fact that this variant represents his “rookie card”—typically a player’s most sought after card. These latter two factors help to explain the extensive interest in the card among broad classes of collectors.

<sup>24</sup> I could have gathered willingness to pay (or willingness to accept) values by auctioning off Thomas cards using an incentive compatible auction institution (i.e., a Vickrey 2<sup>nd</sup> price auction), but market prices should influence bids, leaving me with a vector of bids that roughly estimate the perceived market price adjusted for transactions costs.

<sup>25</sup> The cheap talk script is similar to List (2001) and notes that:

In most questions of this kind, folks seem to have a hard time doing this. They act differently in a hypothetical situation, where they don’t really have to pay money, than they do in a real situation, where they really have to pay money. We call this “hypothetical bias”. “Hypothetical bias” is the difference that we continually see in the way people respond to hypothetical situations as compared to real situations. So, if I was in your shoes, and I was asked to make a choice, I would think about how I feel about spending my money this way. When I got ready to choose, I would ask myself: if this was a real situation, do I really want to spend my money this way?



microscopic inspection would rate a perfect “10”, while defects, including minor wear on the corners, would decrease the card’s grade to a “7”. The card’s overall grade is computed via the aggregation of the various characteristics.

PSA (Professional Sports Authenticators) is the industry leader in grading services, and its parent company became publicly traded in 1999 (Collectors Universe, under NASDAQ ticker symbol CLCT). PSA has graded more than 7 million sportscards since its inception in 1987. Professional grading is voluntary and costs \$6-\$100 per card, depending on package size and requested turnaround time. Importantly, the fee is independent of the actual grade received. Graded cards are encased in plastic and sealed with a sonic procedure that makes it virtually impossible to open and reseal the case without evidence of tampering.

PSA adopted integer grades from 1 to 10, where a “10” is considered Gem Mint and commands a premium price. A PSA “9” card is considered Mint and is the next most valuable card type. As witnessed by the  $c(q)$  and  $v(q)$  vectors used in Treatments I and II, card values are convex in the grade received. Importantly, Jin et al. (2004) provide evidence suggesting that even under PSA’s coarse grading system, certification reveals important information to ordinary consumers. Yet they report that dealers gain no information from a card’s PSA grade, suggesting that dealers are able to evaluate quality as well as PSA.

Sports tickets and ticket stubs have recently gained enough market acceptance to merit professional grading. Ticket supply, of course, depends on the stadium size of the event and the proportion of fans in attendance that preserved their ticket stubs (or in the case of unused tickets, the number of fans who left their tickets unused). Ticket grading is similar to sportscard grading: an identical 10-point scale is used, and sharpness of corners, centering of ticket, sharp focus, and original gloss are very important. Furthermore, staining, printing imperfections, and print quality of crucial game information are also important in determining ticket quality.

## Appendix D. Alternative Estimation Strategy: Panel Data Ordered Probit Model

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I begin by coding dealer behavior for quality: “top quality—PSA 10” “nice quality—PSA 9,” etc. Considering these classifications as a ranking of “the propensity to provide quality,” I build a model around a latent regression of the form:

$$Q^* = Z'\beta + \varepsilon, \quad (4)$$

where  $Q^*$  is the unobserved vector of “propensity to provide quality,”  $Z$  is a vector of dealer-specific variables that also includes  $p$ ,  $\beta$  is the estimated response coefficient vector, and  $\varepsilon_i$  is the well-behaved random error component. Although I do not directly observe  $Q^*$ , I do observe an approximation of  $Q^*$ :

$$Q = \begin{cases} 6 & \text{if } Q^* \leq 0 \\ 7 & \text{if } 0 < Q^* \leq \phi_1 \\ 8 & \text{if } \phi_1 < Q^* \leq \phi_2 \\ 9 & \text{if } \phi_3 < Q^* \leq \phi_4 \\ 10 & \text{if } \phi_4 < Q^* \leq \phi_5, \end{cases} \quad (5)$$

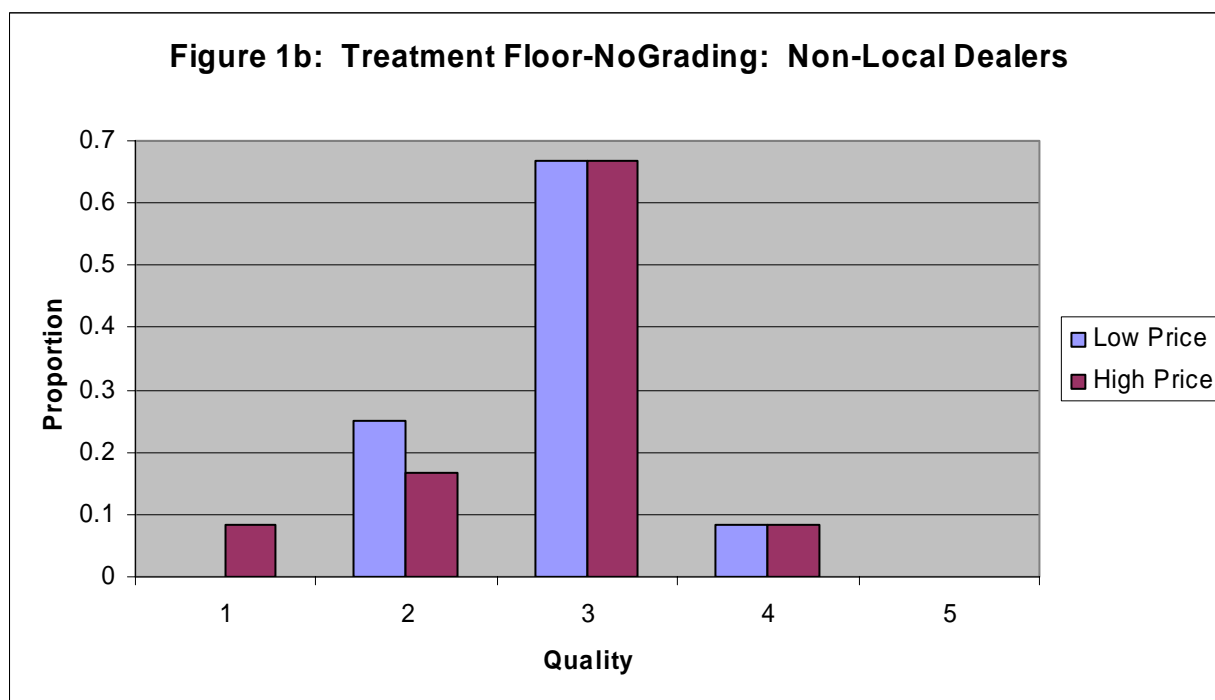
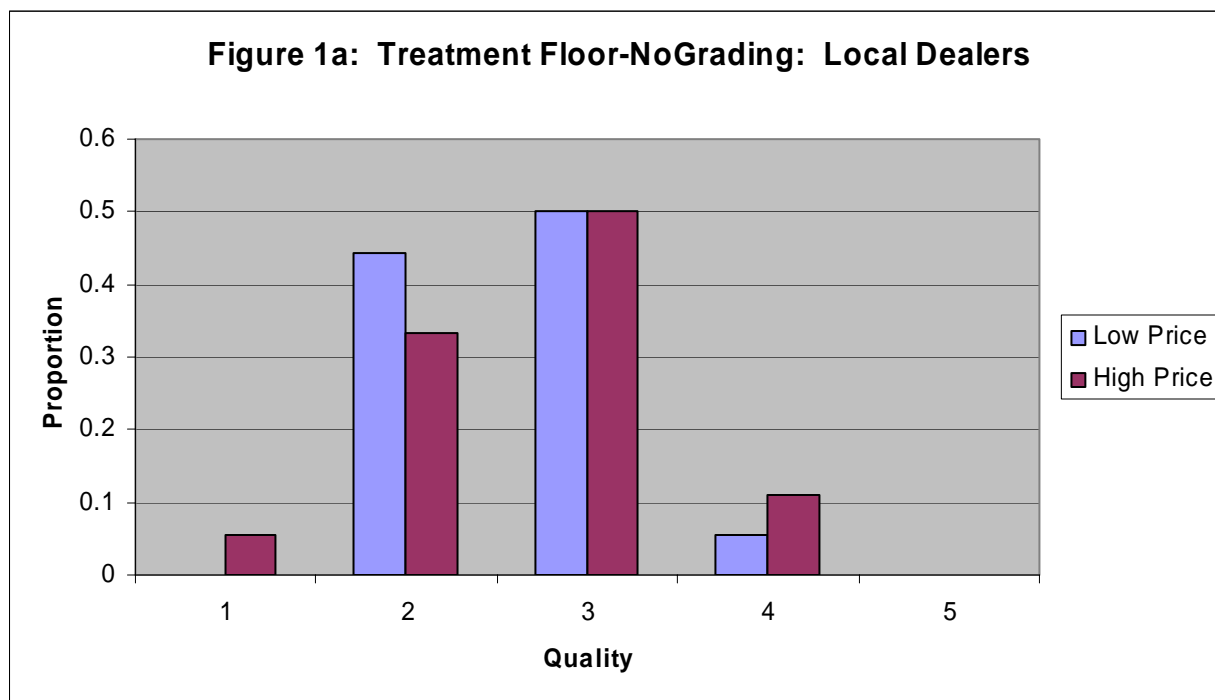
where  $\phi_i$  are unknown parameters that are estimated jointly with  $\beta$ . As such, when estimating this model one obtains threshold levels of “propensity to provide quality” by measuring how variables in vector  $Z$  affect ranked responses,  $Q^*$ .

A few aspects of this particular estimation procedure merit further consideration. First, since the  $\phi$ ’s are free parameters, there is no significance to the unit distance between the set of observed values of  $Q$ , thus avoiding symmetric treatment of one-unit changes in the dependent variable. Second, estimates of the marginal effects in the ordered probability model are quite involved because there is no meaningful conditional mean function. I therefore compute the effects of changes in the covariates on the  $\gamma$  probabilities:  $\partial \text{Prob}[\text{cell } \gamma] / \partial Q = [f(\phi_{j-1} - Z'\beta) - f(\phi_j - Z'\beta)] * \beta$  -- where  $f(\bullet)$  is the standard normal density, and other variables are defined above. By definition, these effects must sum to zero since the probabilities sum to one.

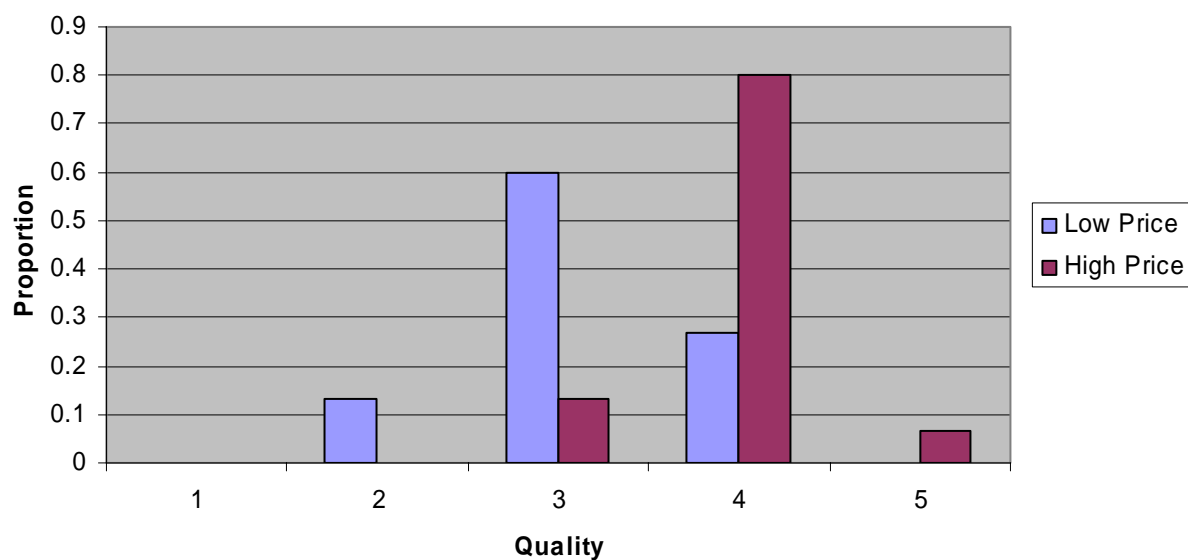
Empirical estimates from these models are available upon request.

## Appendix E. Supplementary Figures

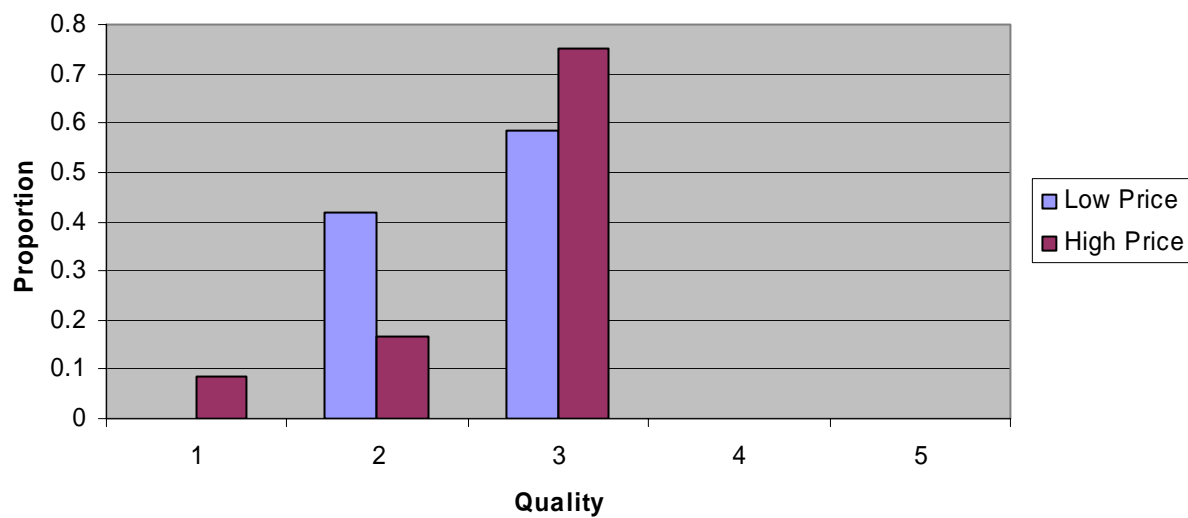
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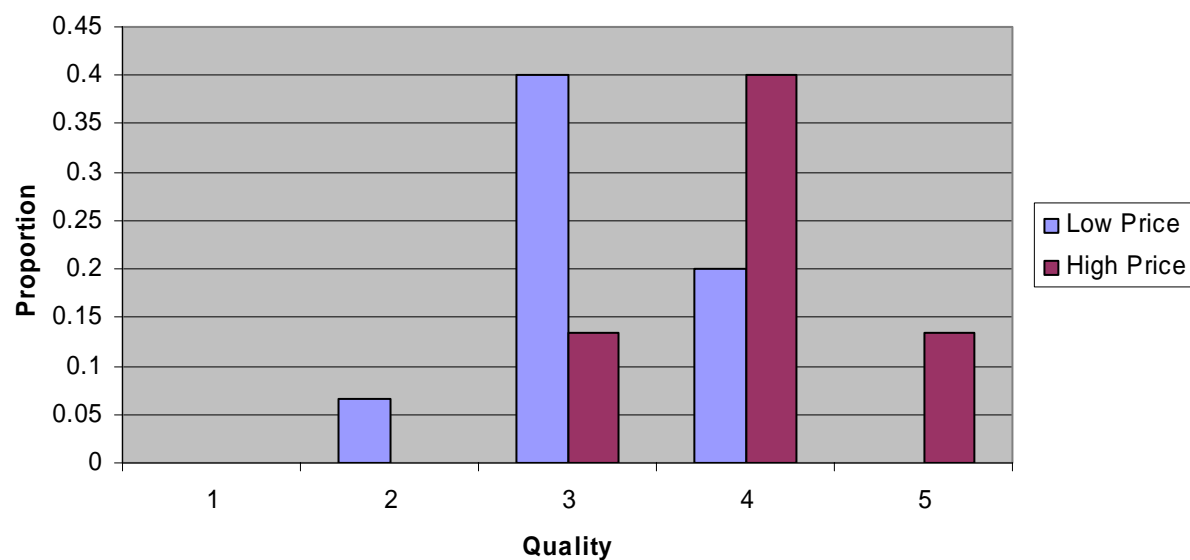
**Figure 2a: Treatment Floor-AnnounceGrading: Local Dealers**



**Figure 2b: Treatment Floor-AnnounceGrading:  
Non-Local Dealers**



**Figure 3a: Treatment Floor-Grading: Local Dealers**



**Figure 3b: Treatment Floor-Grading: Non-Local Dealers**

